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The Relation between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1862—1957:

A Further Analysis[1]

By Richard G. Lipsey

In an earlier paper in this journal, Professor Phillips[2] has advanced the hypothesis that the percentage rate of change of money wage rates in the United Kingdom ($\dot{W}$) can be explained to a very large extent by: (i) the percentage of the labour force unemployed (U), and (ii) the *rate of change* of unemployment ($\dot{U}$). After an inspection of the data, Phillips concluded not only that there is a clearly observable relationship between these variables, but that the form of the relationship has been remarkably stable over a period of almost one hundred years. The purpose of the present paper is to reconsider Phillips' work in some detail. In particular it seemed necessary: (i) to consider the general theoretical model that is being tested; (ii) to quantify Phillips' results, determining, if possible, the proportion of the variance in money wage rates that is associated with the two variables, level of unemployment (U) and rate of change of unemployment ($\dot{U}$); (iii) to provide systematic tests of the various subsidiary hypotheses framed by Phillips during the course of his analysis; and (iv) to test hypotheses that follow from possible alternative models. The logical order in which to deal with these topics, in the absence of Phillips' paper, would be, first, to outline the phenomena which require explanation, then to develop a model which will explain the phenomena, and, finally, to test further implications of the model. Given Phillips' paper, however, a slight change of approach seems to be desirable. In the first section of this paper a report is given of the statistical analysis carried out on data for the period 1862–1913. Although the main purpose is to discover what phenomena require explanation, a rather elaborate treatment is required in order to test the hypotheses about these phenomena framed by Phillips. This is

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[1] The present paper, like Professor Phillips', is a part of a wider research project financed by the Ford Foundation. The writer was assisted by Mr. Peter Lantos and Mrs. June Wickins. This paper was the subject of extended discussion at the LSE Staff Seminar on Methodology and Testing in Economics and I am indebted to all the members for many comments and suggestions; I also benefited from the discussion at the University of Manchester Advanced Economics Seminar where some of the material embodied in this paper was first presented. Mr. F. Brechling and Dr. S. F. Kaliski have given valuable criticisms and I am particularly indebted to Professor Phillips for his constant aid and encouragement.

[2] A. W. Phillips, " The Relation Between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861–1957 ", *Economica*, Nov. 1958.

necessary in order to build up a clear picture of our explicanda. Although many of Phillips' subsidiary hypotheses are rejected, the data are shown to support Phillips' main contention that there is a significant relation between the rate of change of money wage rates and the level and the rate of change of unemployment. Having established the evidence for these relations, the second section is devoted to the construction of a theoretical model which adequately accounts for them. Phillips had given very little indication of the sort of model of market behaviour which would produce his postulated relations. The third section is devoted to an analysis of the data for the post–1918 period. The theory developed in Section II is particularly useful in interpreting the differences which occur between the relations existing in the nineteenth century and in the twentieth century.

I

THE PERIOD 1862–1913

1. *The relation between the rate of change of money wage rates* ($\dot{W}$) *and the level of unemployment* (U)[1]

The unemployment figures used by Phillips showed the percentage of the unionized labour force unemployed, while the figures for the rate of change of wage rates were calculated[2] from the Phelps Brown-Hopkins index.[3]

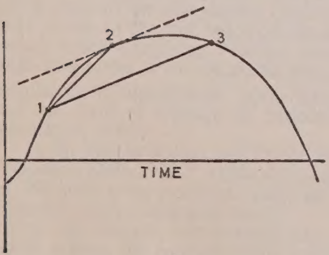

Fig. 1.

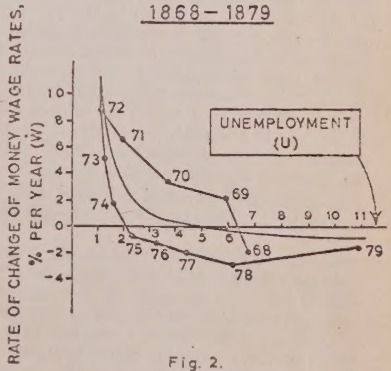

Fig. 2.

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[1] Since the *level* of unemployment is uncorrelated with the *rate of change* of unemployment, as is any trend-free variable with its own rate of change, the relation between the rate of change of money wages and each of the independent variables, U and $\dot{U}$, can be considered separately. The actual r^2 for U and $\dot{U}$ is ·0002.

[2] Phillips, *ibid.*, p. 290, n. 1, took half the first central differences ($W_{t+1} - W_{t-1} \div 2$) as the best approximation to the absolute rate of change of wages. The argument for approximating a continuous derivative by this method rather than by the more intuitively plausible method of taking the difference between this year's wage index and last year's ($W_t - W_{t-1}$) can best be explained by reference to the diagram. Fig. 1 shows a continuous time series (say one for the rate of change of wages). Only a discrete number of regularly-spaced observations are available, say those at 1, 2, and 3, and it is desired to approximate the derivative at 2 (the true value being given

Fig. 3 shows the data for $\dot{W}$ and U for the period under consideration. Phillips elected to describe the data by a curve of the type

$$\dot{W} = \alpha + \beta U^{\gamma} \ldots\ldots\ldots\ldots \quad (1a)$$

$$\text{or} \quad \log(\dot{W} - \alpha) = \log \beta + \gamma \log U \ldots\ldots\ldots\ldots \quad (1b)$$

where $\dot{W}$ is the rate of change of money wage rates $\left(\dot{W}_t \equiv \frac{W_{t+1} - W_{t-1}}{2W_t} \right)$, and U is the percentage of the labour force unemployed. This curve could not be fitted to all 52 observations because points below the asymptote ($\dot{W} < \alpha$) would require negative logarithms. Hence Phillips grouped his observations into six class intervals based on the level of unemployment[1] and found the mean values of $\dot{W}$ and U for each of the six groups. Having thus compressed his data into six points, he fitted his curve to these points, using a trial-and-error procedure, and obtained the following equation:

$$\dot{W} = -0.9 + 9.638\, U^{-1 \cdot 394}, \ldots\ldots\ldots\ldots \quad (2)$$

which is plotted as curve (2) in Fig. 3.

Since, for purposes of the present study, it seemed desirable to treat the data by standard statistical methods if at all possible, a new

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by the slope of the broken line tangent to the curve at 2). Taking the rate of change to be equal to the difference between the values of the function at 2 and at 1 is equivalent to estimating the derivative at 2 to be equal to the slope of the line joining 1 and 2. But the slope of this line is typical of the value of the derivative somewhere *between* 1 and 2, so that this method gives the derivative somewhere *between* the two points of time and is thus equivalent to introducing a time lag of approximately six months into the rate-of-change series. On the other hand, taking half the first central difference is equivalent to estimating the derivative to be equal to the slope of the line joining 1 and 3. In a regular curve this latter value is likely to be closer to the true value of the derivative at 2 than is the former value. In a recent article criticising Phillips' work, Mr. Routh has argued that the actual wage rate series is too crude to make the difference between the two methods of calculating $\dot{W}$ significant. (Guy Routh, " The Relation Between Unemployment and the Rate of Change of Money Wage Rates : A Comment ", *Economica*, November, 1959.)

[3] E. H. Phelps Brown and Sheila Hopkins, " The Course of Wage Rates in Five Countries, 1860–1939 ", *Oxford Economic Papers*, June 1950. Mr. Routh (*loc. cit.*, pp. 299–305) gives a detailed study of the coverage of the wage rate and the unemployment series and argues that " . . . in the two series used by Professor Phillips, neither the weights, occupations nor the industries are a good match (p. 303) ". Routh argues, for example, that any fixed weighted index of rates will not allow for movements between areas and occupations. This is undoubtedly correct. It is, however, always possible to show that any set of statistics are not perfect or even, by some absolute standard, that they are downright bad. The relevant question is not whether the figures are perfect, but whether they are good enough for the purposes at hand. The question of whether or not the postulated relation is strong enough to show up in spite of imperfections in the data, can only be answered by the empirical results: in this case the postulated relation is strong enough. Another criticism of Phillips' article is to be found in K. G. J. C. Knowles and C. B. Winsten, " Can the Level of Unemployment Explain Changes in Wages ? ", *Bulletin of the Oxford Institute of Statistics*, May 1959.

[1] The class intervals (percentage unemployment) with the number of items contained in each class given in parentheses are: 0—2 (6), 2—3 (10), 3—4 (12), 4—5 (5), 5—7 (11), 7—11 (9) (the upper limit is included in each class).

equation was adopted which could be fitted to all the 52 original observations:[1]

$$\dot{W}=a+bU^{-1}+cU^{-2}. \qquad (3)$$

It was found that, by suitable choice of the constants b and c, this curve could be made to take up a position virtually indistinguishable

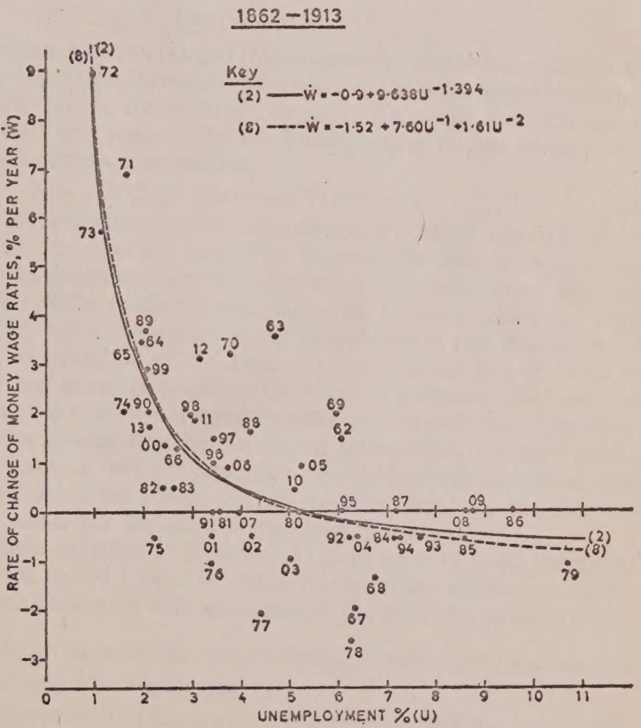

Fig. 3.

from that taken up by curve (1) for any value of γ between -1 and -2. Thus choosing between the two curves does not necessitate choosing between different hypotheses about the nature of the relation between $\dot{W}$ and U.[2]

The curve was first fitted to Phillips' six points of averages and gave the equation

$$\dot{W}=-0.44+0.023U^{-1}+12.52U^{-2}. \qquad (4)$$

---

[1] For purposes of the present section the shape of the relation assumed by Phillips is accepted so that the problem is merely to find an equation which takes the same shape as equation (1) but which can be fitted by least squares. In Section II the general form of the relationship between $\dot{W}$ and U is considered in some detail.

[2] When (1) and (3) are fitted to the same data, normal least squares fitting does, however, result in slightly different shapes to the two curves because in one case the sum of the squares of the residuals expressed in logarithms is minimised while, in the second case, it is the sum of the squared residuals expressed in natural numbers that is minimised.

The difference[1] between equations (2) and (4) results from the procedure of minimising the squares of the differences between the actual and the estimated values expressed in logarithms for (2) and in natural numbers for (4).[2] Next the curve was fitted to Phillips' original 52 observations for the years 1862–1913 which resulted in the following equation:

$$\dot{W} = -1.14 + 5.53U^{-1} + 3.68U^{-2}. \ldots\ldots\ldots\ldots \quad (5)$$

The difference between (4) and (5) indicates the distorting effect caused by fitting to points of averages rather than to the original observations.[3]

Next the Phelps Brown-Hopkins series for wage rate changes in 1881 to 1885 was replaced by the Bowley series for the same years.[4] The fitted equation then became

$$\dot{W} = -1.42 + 7.06U^{-1} + 2.31U^{-2} \ldots\ldots\ldots\ldots \quad (6)$$

There is a noticeable shift in the relationship when equation (5) is replaced by equation (6) and there is room for debate as to which series for the disputed years, and thus which equation, should be used. The Bowley series conforms with the pattern seen in the other eight cycles which cover the period under consideration and thus seems to be the more plausible of the two. In the absence of any evidence favouring one series rather than the other, we cannot eliminate one merely because it does not conform with our hypothesis. Therefore, although the Bowley substitution for the years 1881–85 is used on the subjective grounds that it seems more plausible, all relations have been recalculated using the Phelps Brown-Hopkins series for the disputed years, the values for the latter being given in footnotes.

The relation specified in equation (6) gives an r^2 of 0.64, indicating that, over the period 1862–1913, 64 per cent. of the variance in money wage rates is associated with variations in the level of unemployment.[5]

2. *The relation between the rate of change of wages ($\dot{W}$) and the rate of change of unemployment ($\dot{U}$)*

After an inspection of his graphs Phillips noted that the relationship between $\dot{W}$ and $\dot{U}$ appeared to be important; he observed that,

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[1] The different fitted relationships may be at least roughly compared by comparing the values of the asymptotes.

[2] Some of the difference is accounted for by the fact that Phillips did not fit to all six points by least squares but rather made his curve go as closely as possible to the two points representing the highest levels of unemployment and then minimised squares on the other four points.

[3] When fitting to points of averages each of the six points is given equal weight although there are considerable differences between the number of items within each class interval.

[4] The Phelps Brown-Hopkins series shows a suspicious stability in wage rates over the period 1881–85 in spite of wide variations in employment, while the Bowley series shows the usual relation with wage rates rising when unemployment falls and then falling as unemployment rises. See Phillips, *loc. cit.*, pp. 287 and 291. Routh (*loc. cit.*, p. 313) has given reasons for the stability in the Phelps Brown-Hopkins index and has argued that this index should *not* be replaced by the Bowley series for these years.

[5] Equation (5) which shows the comparable relation determined without the substitution of Bowley's index for 1881–85 gives an $r^2 = 0 \cdot 64$.

compared to the value predicted by the relation between $\dot{W}$ and U, $\dot{W}$ tended to be high when unemployment was falling ($\dot{U}<O$) and low when unemployment was rising ($\dot{U}>O$). In other words, the change in money wage rates is greater than would otherwise be expected when unemployment is *falling* and less than would otherwise be expected when unemployment is *rising*. He did not, however, attempt to determine either the precise form of the relationship between $\dot{W}$ and $\dot{U}$ or its quantitative significance. Fig. 2 shows the relation between $\dot{W}$ and U for the years 1868–79 together with the curve described by equation (2). This general picture is typical of the nineteenth century cycles. The "loop" is clearly observable with the actual $\dot{W}$ being above the fitted curve when unemployment is falling and below the curve when it is rising.

It was now desired to measure this relationship which was very strongly suggested by inspection. Half the first central difference was taken as the best approximation to the rate of change of unemployment in any year.[1] Thus, a new variable was defined, $\dot{U}_t \equiv \frac{U_{t+1}-U_{t-1}}{2U_t}.\ 100$, and the new regression equation became

$$\dot{W}=a+bU^{-1}+cU^{-2}+d\dot{U} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (7)$$

which, when fitted to the original observations, gave

$$\dot{W}=-1.52+7.60U^{-1}+1.61U^{-2}-.023\dot{U} \quad \ldots\ldots\ldots\ldots \quad (8)$$

Curve (8) in Fig. 3 shows this relation when $\dot{U}=O$. R^2 for this relationship is 0.82 while the squared partial correlation coefficients are 0.78 for U and 0.50 for $\dot{U}$. This indicates that 82 per cent. of the variance in $\dot{W}$ can be associated with variations in U and $\dot{U}$, and that $\dot{U}$ can remove 50 per cent. of the variance not already associated with U while, if $\dot{U}$ is considered first, U is associated with 78 per cent. of the residual variation in $\dot{W}$.[2]

Now that the influence of $\dot{U}$ has been measured it is possible to check quantitatively on Phillips' observation that ". . . it appears that the width of the loops obtained in each trade cycle has tended to narrow, suggesting *a reduction in the dependence of the rate of change of wage rates on the rate of change of unemployment*".[3] This statement is taken to mean that, throughout the period, any given rate of change of unemployment was associated with a progressively diminishing rate of change of money wages (i.e., that, if it were fitted separately for

---

[1] See p. 2, n. 2.

[2] The relations without the Bowley substitutions 1881–85 are as follows: $\dot{W}=-1\cdot 23+6\cdot 00U^{-1}+3\cdot 05U^{-2}-\cdot 021\dot{U}$, $r^2=0\cdot 79$. The squared partial correlation coefficient for $\dot{U}$ of 0·41 is smaller than the one quoted in the text. As would be expected the substitution of a series without a loop for a series with a loop results in a reduction of the explanatory power of $\dot{U}$.

[3] Phillips, *loc. cit.*, p. 292, italics added. After making this observation, Phillips offers two possible explanations to account for the supposed change.

each cycle, the parameter d in equation (7) would diminish from cycle to cycle). In order to check this statement equation (6) was used to predict values for $\dot{W}$ and the differences between these predicted values and the observed values were plotted on a scatter diagram against $\dot{U}$.[1] A separate diagram was drawn and a straight line (i.e. $R_{(6)} = a + b\dot{U}$) was fitted for each cycle. The slope of the line is an index of the width of Phillips' loops. The values in Table 1 show that, in the cycle of 1893–1904, for example, a 100 per cent. increase in the percentage of the working force unemployed (e.g. from 3 per cent. unemployment to 6 per cent.) was associated with an observed figure for the rate of change of wage rates 2.2 per cent. below the value predicted by equation (6). The value of the r^2 indicates the importance of $\dot{U}$ as an additional explanatory variable in each cycle. An inspection of Table 1 reveals that there is considerable variation in the value of the coefficient "b" from cycle to cycle but that there is no clear

TABLE 1

STRAIGHT LINES RELATING RESIDUALS FROM EQUATION (6) TO THE RATE OF CHANGE OF UNEMPLOYMENT

$R_6 = a + b\dot{U}$

Period(2)	b	r^2
1862–68	·017	·25
1868–79	·046	·91
1879–86	·015	·56
1886–93	·016	·91
1893–1904	·022	·59
1904–09	·011	·49

---

[1] Since the units in which all the variables are expressed are percentage points there is the possibility of confusion when residuals are calculated. To avoid such confusion it may be worthwhile defining all the variables and the residuals at this point:

(i) the rate of change of money wage rates at time $t \equiv$

$$\dot{W}_t \equiv \frac{W_{t+1} - W_{t-1}}{2W_t} . 100;$$

(ii) the percentage of the labour force unemployed at time $t \equiv U_t$;

(iii) the rate of change of unemployment at time $t \equiv$

$$\dot{U}_t \equiv \frac{U_{t+1} - U_{t-1}}{2U_t} . 100;$$

where W_t is the index of money wage rates. Since lagged variables are not used in this section, the time subscript t is dropped from all the variables in the equations.

(iv) the deviation of the observed value of $\dot{W}$ from the value predicted from equation $n \equiv R_n \equiv \dot{W}_o - \dot{W}_{en}$ where o stands for observed, and en for estimated, from equation n. R_n is always expressed in original units which, in the case of $\dot{W}$, are percentage points. Thus a residual of +1 per cent. might mean that the actual $\dot{W}$ was 3 per cent. while the estimated value was 2 per cent. R_n is always used as defined above and *never* as a proportional residual (i.e. R_n per cent. $\equiv \frac{\dot{W}_o - \dot{W}_{en}}{\dot{W}_{en}}$ is never used).

[2] To make the figures comparable with those of Phillips, the last year of each cycle has also been included as the first year of the subsequent cycle. The years 1910–13 are excluded because they do not constitute a complete cycle.

evidence that it becomes progressively smaller cycle by cycle. The cycle 1868–79 is unusual in that the deviations of the observed from the predicted values of $\dot{W}$ associated with any given level of $\dot{U}$ are three times as large as those associated with most other cycles. At the 5 per cent. probability level there is a significant difference between the coefficient b for the cycle 1868–79 and those for all other cycles, while the coefficients for the other cycles do not differ significantly from one another. Thus there is some evidence that the loop for 1868–79 is significantly wider than all the other loops, while there is no evidence of significant variations in widths of loop between the other cycles. A hasty comparison of the loop for the period 1868–79 with those that came afterwards may have led Phillips to the erroneous conclusion that the loops were getting progressively narrower.[1]

We must conclude therefore that there is no clear evidence in favour of the hypothesis that $\dot{U}$ is a variable whose importance was diminishing over the period. There is thus no need to attempt the sort of explanation given by Phillips.

3. *Consideration of effects of changes in the cost of living as an additional explanatory variable*

Phillips advanced the hypothesis that cost of living adjustments affect money wage rates with a threshold effect. If wage rates would have risen by X per cent. in the absence of any changes in the cost of living, then an increase of up to X per cent. in the cost of living will have no effect on wages ". . . for employers will merely be giving under the name of cost of living adjustments part of the wage increases which *they would in any case have given as a result of their competitive bidding for labour* ".[2] If, however, the cost of living rises by *more than X per cent.*, then this will also cause wages to rise by more than X per cent., i.e., by more than they otherwise would have done. This implies that the outcome of the wage bargain is unaffected by any change in the cost of living unless it actually threatens to reduce real wages, so that active and at least partially successful attempts must be made to push up money wage rates in response to price level changes that actually threaten to lower real wages. It also implies, however, either that unions passively accept any change in the price level which threatens to take away anything less than 100 per cent. of the increase in real wages that could have resulted from a rise in money wages, or that any attempts to resist such losses are totally frustrated by employers. This behaviour may seem intuitively implausible but it is necessary to see if the data provide any evidence for it.

---

[1] The same experiment was made relating $\dot{U}$ to the residuals from Phillips' own equation ($R_{(2)}$) with similar results to those given in the text. Thus the rejection of Phillips' hypothesis is not the result of the adoption of a new equation.

[2] Phillips, *loc. cit.*, p. 284, italics added. Phillips gives no reason for believing that cost-of-living adjustments operate in this manner.

In order to test Phillips' hypothesis two series were computed. First, the residuals $R_{(8)}$ were calculated. This series showed whether actual money wage rates had risen by more or by less than the amount associated with the existing levels of U and $\dot{U}$. The second series was the change in the real wage rate which was computed by adjusting the change in the money wage rate for the change in the retail price index.[1] A scatter diagram was then drawn relating the residuals, $R_{(8)}$, to the change in the real wage rate. The Phillips hypothesis predicts that when the real wage actually fell, the observed rise in money wage rates would be greater than the predicted rise, but it says nothing about what happens when the real wage rate rises.

In the period under consideration there were fifteen years in which the real wage fell (i.e. when the cost of living increase from the previous year to the present one was more than the increase in money wage rates). In only five of these years was the increase in money wage rates more than that predicted from the equation relating $\dot{W}$ to U and $\dot{U}$, and in none of these years was the deviation more than one half of 1 per cent.[2] In other words, of those years in which the real wage fell there was not one in which the money wage rate rose by more than one half of 1 per cent. more than was predicted by equation (8). In ten of the years in which the real wage fell the rise in money wage rates was *less* than that predicted by equation (8). Thus we must conclude that the evidence does not support Phillips' hypothesis that the cost of living affects wage rates only with a threshold effect.[3]

The rejection of Phillips' hypothesis suggests that it may be desirable to consider a simpler cost of living hypothesis. This hypothesis is that the outcome of the wage bargain is affected simply by the *change* in the cost of living, that an increase in the cost of living makes trade unions more aggressive in demanding increases and employers and arbitrators more willing to grant them, while a decrease in the cost of living acts in the reverse direction. This hypothesis predicts simply that deviations of actual wage increases from those predicted by equation (8) would be associated with the change in the cost of living index, increases in the cost of living being associated with positive deviations and decreases with negative deviations.

As a first check on this hypothesis the residuals from equation (8) were plotted against the percentage change in the cost of living index. In 37 of the 52 years under consideration the residual, R_8, was not more than 1 per cent ($-1 < R_8 < +1$). Of the eight years in which

---

[1] The index used was the retail price index taken from Phelps Brown-Hopkins, *loc. cit.*

[2] The same experiment was made, using Phillips' equation (2), to estimate the values of $\dot{W}$, and the results were substantially the same as those reported in the text.

[3] One other possibility is that there might be a time lag in this process so that decreases in the real wage rate at year t would be followed by abnormally high increases in money wage rates in year $t+1$. The one year lag, however, produces results comparable to those quoted in the text.

there was a positive residual of more than 1 per cent $(R_8 > +1)$, six were years in which the cost of living rose. Of the eight years in which there was a negative residual greater than 1 per cent $(R_{(8)} < -1)$, seven were years in which the cost of living fell while only one was a year in which the cost of living rose. This suggested that, if there was any relation between cost of living changes and wage rate changes, it was a simple one, $\dot{W}$ being related in a straightforward manner to changes in the cost of living. The degree of scatter was, however, very large; there were, for example, eight years in which the cost of living changed by more than 2 per cent. while the actual wage rate change was within half of 1 per cent. of the value predicted by equation (8).

In order to check further on the quantitative significance of cost of living changes as an additional explanatory variable, equation (8) was amended by adding a term for the percentage change in the cost of living index. The equation then became

$$\dot{W}=a+bU^{-1}+cU^{-2}+d\dot{U}+e\dot{P} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (9)$$

where $\dot{P}$ is the percentage change in the cost of living index,[1] $\dot{P}_t \equiv \frac{P_{t+1}-P_{t-1}}{2P_t} . 100$. When fitted to the data for the years 1862 to 1913 this becomes:

$$\dot{W}=-1.21+6.45U^{-1}+2.26U^{-2}-.019\,\dot{U}+.21\dot{P} \quad \ldots\ldots \quad (10)$$

A comparison of equations (8) and (10) shows that the addition of a cost of living variable causes the curve relating $\dot{W}$ to U (when $\dot{U}=\dot{P}=O$) to shift upwards for levels of U greater than 3 per cent. and less than 1 per cent., while, between 1 and 3 per cent., the curve shifts slightly downwards. The (small) coefficient of $\dot{P}$ indicates that an increase of almost 5 per cent. in the cost of living is associated with an increase in money wage rates of only 1 per cent. Finally, the R^2 for this relation is 0.85 while the squared partial correlation coefficient for $\dot{P}$ is 0.17, indicating that 17 per cent. of the variance in $\dot{W}$ which remains after allowing for U and $\dot{U}$ can be removed by associating $\dot{W}$ with $\dot{P}$.[2]

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[1] $\dot{P}_t \equiv \frac{P_t - P_{t-1}}{\frac{1}{2}(P_t+P_{t-1})}$ was tried as an alternative cost of living variable thus introducing a six-months' time lag on cost of living adjustments. The results were broadly similar, but the correlations slightly lower.

[2] The standard error of estimate of $\dot{W}=\cdot 86$, while standard errors for the regression coefficients are $b=2\cdot 12$, $c=2\cdot 13$, $d=\cdot 004$, $e=\cdot 07$. All of the partial correlation coefficients are significant at the 5 per cent. level and there is no evidence of significant auto-correlation in the $\dot{W}$ residuals for time lags of one to four periods at the 5 per cent. probability level. The size of the standard errors for b and c may be misleading because quite large changes can be made in these coefficients without causing large shifts in the curve relating $\dot{W}$ to U.

The comparable relations without the Bowley substitution for 1881–85 are as follows:

$$\dot{W}=-0\cdot 94+4\cdot 92U^{-1}+3\cdot 66U^{-2}-\cdot 016\dot{U}+0\cdot 20\dot{P},$$

$r^2=0\cdot 82$.

Finally, it was desired to see if $\dot{P}$ could be an alternative explanatory variable to either U or $\dot{U}$. The most plausible hypothesis here seemed to be that $\dot{U}$ and $\dot{P}$ might be very highly correlated since retail prices would tend to rise on the upswing of a trade cycle and fall on the downswing. Thus the loops relating $\dot{W}$ to $\dot{U}$ might be merely a reflection of cost of living changes over the cycle. There is in fact very little relation between $\dot{U}$ and $\dot{P}$; the squared coefficient of correlation between $\dot{U}$ and $\dot{P}$ is only 0.19. In order to see which is the better explanatory variable, $\dot{U}$ or $\dot{P}$, equation (9) was amended by dropping the term for $\dot{U}$, thus producing:

$$\dot{W} = -.90 + 5.23U^{-1} + 3.20U^{-2} + .37\dot{P} \quad \ldots\ldots\ldots\ldots \quad (11)$$

R^2 for this relation is 0.76. The squared partial correlation coefficient for $\dot{P}$ is .33, which compares with .50 for $\dot{U}$ when the effect of U is already allowed for. This indicates that $\dot{P}$ has only about two-thirds the explanatory value of $\dot{U}$ when they are considered as alternative variables to be added to the effect of U. The other possible situation would be to use $\dot{P}$ as the sole explanatory variable so that $\dot{P}$ would be an alternative for U. A linear relation between $\dot{W}$ and $\dot{P}$ produced the equation $\dot{W} = 1.14 + .55\dot{P}$ and $r^2 = 0.27$.

4. *The special explanation of 1893–96*

Phillips singled these years out for a special *ad hoc* explanation, apparently believing that the residuals were especially large or particularly significant in these years. He suggested that this could be accounted for by the growth of employers' federations in the 1890's and resistance to trade union demands from 1895 to 1897. Whatever may have been the industrial history of the period, there is no empirical evidence of *exceptional* downward pressure on wages. Estimated values for the change in money wage rates were calculated from equation (10) for the period 1894–96. The wage rate change in 1894 was actually one-third of 1 per cent. higher than the change predicted from equation (10); in 1895 it was only one-third of 1 per cent. less than that predicted by the equation, while in 1896 the actual rise (1 per cent.) was only eight-tenths of 1 per cent. less than the predicted rise (1.87 per cent.). Such very small deviations of the actual from the predicted values can hardly be regarded as significant; larger deviations than that occurring in 1896 were observed in no less than 21 of the 52 years under consideration. We must conclude, therefore, that there is no need for a special explanation of the observed events of 1893–96 which in fact conformed quite closely to the general pattern of the whole period.

5. *Conclusions*

(i) There is a significant relation between the rate of change of money wage rates on the one hand and the level of unemployment

and its rate of change on the other. Over 80 per cent. of the in money wage rates over the period 1862–1913 can be assoc with these two variables, U and $\dot{U}$. (ii) The Phillips hypothe the influence of the rate of change of unemployment has di over the period is rejected. (iii) The Phillips hypothesis cost of living enters with a threshold effect is rejected. There seem be some evidence in favour of a simple (but rather weak) rela between changes in the cost of living and changes in money w rates. (iv) There is no evidence of a *need* for any special explana of the years 1893–96.

II

THE MODEL[1]

The analysis reported in Section I shows that there is a signifi relation between $\dot{W}$, U, and $\dot{U}$, and it is now necessary to constru theoretical model that will satisfactorily account for the relations It is necessary to take this step for at least three reasons. First relation between $\dot{W}$, U, and $\dot{U}$ is open to serious misinterpreta and such misinterpretations can be prevented only when the m which underlies the relation is fully specified. Second, if the rela ceases to hold, or changes, and we have no model to explain it, can only say "the relation has ceased to hold" or "the relation changed" and we will have learned nothing more than this. If have a model explaining the relationship, we will know the conditi under which the relation is expected to remain unchanged. Ther a change occurs, the model will predict *why* this has happened this prediction will give rise to further tests from which we can lea Third, unless it is a very *ad hoc* one, the model will give rise to furt testable predictions in addition to the relation between the th variables $\dot{W}$, U, and $\dot{U}$, and from the testing of these we will g further relevant information.[2]

1. *The relation between $\dot{W}$ and U*

We shall consider this relationship, first, for a single market, then for the whole economy, using lower-case letters to refer to single market variables and capitals to refer to the correspond macro-variables.

We might analyse the market for any commodity since the argum at this stage is quite general. Since, however, the subject of

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[1] I am particularly indebted to Mr. G. C. Archibald whose persistent critic of measurement without adequate theory have been to a very great extent respons for the whole of Section II. He should in fact be regarded as joint author of (1) of this section.

[2] The relation between $\dot{W}$, U and $\dot{U}$ is already known and the model wil specifically constructed to account for it. Thus to *test* the model against existing observations of these variables is to conduct a "sun-rise test", tha to test the theory by checking some prediction which has a zero chance of b found wrong.

present article is the labour market we shall use the terminology appropriate to that market. The usual argument merely states that when there is excess demand, for example *ij* in Fig. 4, wage rates will rise, while, when there is excess supply, for example *mn* in Fig. 4, wage rates will fall. Nothing is said about the speed at which the adjustment takes place. We now introduce the dynamic hypothesis that the rate at which w changes is related to the excess demand, and specifically, the greater is the proportionate disequilibrium, the more rapidly will wages be changing.[1] Thus the hypothesis is $\dot{w} = f\left(\frac{d-s}{s}\right)$, which says that the speed at which wages change depends on the excess demand as a proportion of the labour force.[2] Fig. 5 illustrates a simple form of this relation, $\dot{w} = \alpha\left(\frac{d-s}{s} \,.\, 100\right)$ according to which if we start with excess demand of, for example, $Oc\left(= \frac{gh}{w'g} \text{ in Fig. 4}\right)$, wages will be rising at the rate *cd*, but, if the excess demand increases to $Oa\left(= \frac{ij}{w''j} \text{ in Figure 4}\right)$, wages will be rising at the rate *ab*.[3]

There are a number of advantages in including the relations illustrated in Fig. 5 in one's theory rather than having only the ones illustrated in Fig. 4. If it is known that both of the curves of Fig. 4 are shifting continuously (e.g. the demand curve due to cyclical variations in income, and the supply curve due to exogenous changes in the labour force), then no two price-quantity observations will lie on the same curve. It will then be difficult to discover by observation the *ceteris paribus* relations either between supply and price or between demand and price. For the relation in Fig. 5 to be observed it is necessary only that there be an unchanging *adjustment mechanism* in the market, i.e., that a given excess demand should cause a given rate of change of price *whatever the reason for the excess demand*—whether demand shift, a supply shift, or a combination of both. The rate of change of price can be observed directly and, to obtain the relation shown in Fig. 5, it is only necessary to know demand and supply *at the existing market price;* it is not necessary to know what would be demanded and supplied at other prices.

Now if excess demand for labour were directly observable there would be no need to go any further. Unfortunately, this is not the

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[1] This is Phillips' hypothesis, *loc. cit.*, p. 283. It is also used extensively, for example, by Bent Hansen, *The Theory of Inflation*, London, 1951.

[2] If we were only concerned with a single market, the hypothesis could be expressed either in absolute or in proportional terms. Inter-market comparisons, however, require a proportionate measure. Consider the elasticity analogy.

[3] The relationship might of course be non-linear, indicating that w increased at either an increasing or a decreasing rate as excess demand increased. The simpler linear relationship is, however, capable of explaining all of the observed phenomena and, in the absence of empirical evidence about the second derivative of $\dot{w}$, the simpler relationship is assumed.

case, at least over a large part of the period under consideration,[1] and it is necessary to relate excess demand to something that is directly observable, in this case the percentage of the labour force unemployed.

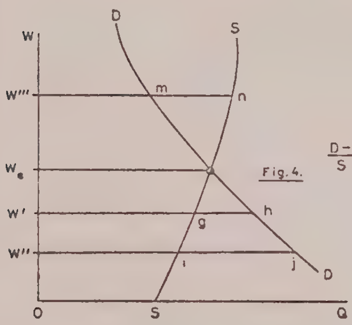

Fig. 4.

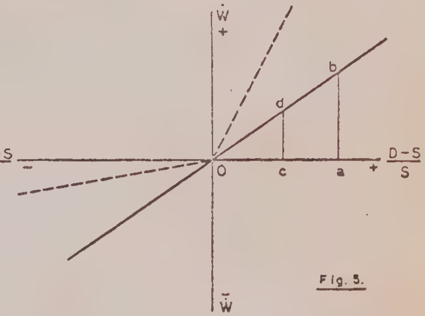

Fig. 5.

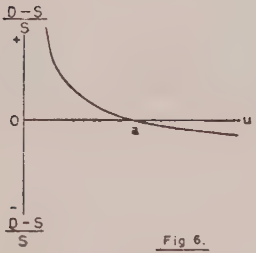

Fig 6.

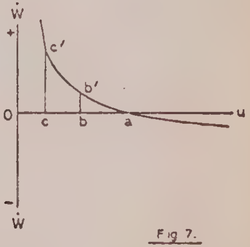

Fig 7.

Fig. 6 shows the relation between $\frac{d-s}{s}$ and the percentage of the labour force unemployed, u. When demand is equal to supply (wage rate Ow_e in Fig. 4), there will be jobs available for all those who wish to work at the going wage rate. This is *not* equivalent to saying that there will be no one unemployed, but rather that the number of unemployed will be matched by an equal number of unfilled vacancies. Given that workers change jobs for any reason whatever, and that a finite time is taken to change, zero excess demand must be accompanied by some positive amount of *frictional unemployment*. From this it follows that, when the wage rate is stable (at Ow_e in Fig. 4), there will be some quantity of unemployment (Oa in Fig. 6), the exact quantity being determined by the amount of movement and the time taken to move. Now consider points to the left of a in Fig. 6. The larger is the excess demand the easier will it be to find jobs, and the less will be the time taken in moving between jobs. Thus, unless there is a

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[1] The difference between unfilled vacancies and unemployed workers might provide a reasonable direct measure of excess demand; but such data are not available for most of the period under consideration.

completely offsetting increase in numbers of persons moving between jobs, an increase in excess demand will cause a reduction in u. It is, however, impossible that u could be reduced below zero so that as $\frac{d-s}{s}$ approaches infinity, u must approach zero (or some small value >0) asymptotically.[1] Now consider points to the right of a. Any increase in excess supply brings an equal increase in the number of persons unemployed. Therefore, to the right of point a, there will be a linear relation between $\frac{d-s}{s}$ and u.[2]

Now in order to obtain the relation between the two observable quantities, $\dot{w}$ and u, we need merely combine Figs. 5 and 6 to obtain the relation illustrated in Fig. 7. The relation between $\dot{w}$ and $\frac{d-s}{s}$ (Fig. 5)

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[1] The following is a simple model which will produce the postulated relationship: *Symbols:* $L \equiv$ labour force $\equiv S$ in Fig. 4, $E \equiv$ number employed, $V \equiv$ number unemployed, $J \equiv$ total jobs available $\equiv D$ in Fig. 4, $N \equiv$ number of unemployed finding jobs, $X \equiv$ proportionate excess demand $\equiv \frac{J-L}{L} \equiv \frac{d-s}{s}$, α and β are two constants.

Assumptions: A constant proportion of those employed, αE, leave employment per unit of time; the number of unemployed who find jobs depends on the number looking for jobs and the number of jobs available: $N = \beta V(J-E)$.

A constant level of V requires: $\alpha E = \beta V(J-E)$.

But $E = L - V$, so $\alpha(L-V) = \beta V(J-L+V)$.

Expanding: $J = \frac{\alpha L}{\beta V} - V + L - \frac{\alpha}{\beta}$. But $X \equiv \frac{J-L}{L}$.

Eliminating J: $X = \frac{\alpha}{\beta V} - \frac{V}{L} - \frac{\alpha}{\beta L}$. ...(*I*)

Differentiating: $\frac{\partial X}{\partial V} = -\frac{\alpha}{\beta V^2} - \frac{1}{L} < 0$, $\frac{\partial^2 X}{\partial V^2} = \frac{\alpha}{\beta V} > 0$.

Therefore an increase in X is associated with a decrease in V but as X increases V falls at a decreasing rate and, from (*I*) above, as $X \to \infty$, $V \to 0$.

[2] There are some reasons for believing that, to the right of a the relation might have a slight curvature which would *increase* as u increased (i.e., $\frac{\partial w}{\partial u} < 0$ *and* $\frac{\partial^2 w}{\partial u^2} < 0$). The excess supply of labour is $\frac{d-s}{s} \cdot 100$ while u is $\frac{d-s}{s} \cdot 100 + F$, where F is the proportion of the labour force frictionally unemployed. If F remains constant as excess supply increases, the relationship between $\frac{d-s}{s}$ and u will be linear. If, on the other hand, $F \to 0$ as $u \to 100$, then the line relating u to $\frac{d-s}{s}$ will *curve* slightly downwards, starting at $u = F$ when $\frac{d-s}{s} = 0$ and reaching $u = \frac{d-s}{s}$ when excess supply is 100 per cent. If F is small (say 5 per cent.), this curvature will be very slight. A second reason is that people in excess supply may not register as unemployed so that recorded u may not increase as fast as real excess supply. With the data used in this study it is impossible to distinguish between $\frac{\partial^2 \dot{w}}{\partial u^2} \lesseqgtr 0$ for high values of u. If, however, it were possible to demonstrate that $\frac{\partial^2 \dot{w}}{\partial u^2} > 0$, we should have to abandon the linear hypothesis illustrated in Fig. 5, at least for situations of excess supply.

is assumed to be linear throughout. The relationship between $\dot{w}$ and u, however, is non-linear to the left of the point a because of the non-linear relation over that range between u and $\frac{d-s}{s}$ (Fig. 6) while the relation between $\dot{w}$ and u is linear to the right of a because of the assumed linear relation over that range between u and $\frac{d-s}{s}$ (Fig. 6). The relation illustrated in Fig. 7 shows the *speed at which prices adjust to a disequilibrium* and we shall call it an *adjustment function.*

This relationship between $\dot{w}$ and u is an extremely simple one, and it holds considerable promise for empirical testing. The relation is, however, easily misinterpreted, and it may be worth considering some examples. Consider, first, a case in which a market is observed over three successive time periods at the points a, b' and c' in Fig. 7. This means that the demand and/or the supply curves have shifted over the period in such a way as to increase the disequilibrium in spite of the increase in wage rates. For example, the demand curve may have shifted so quickly to the right that the equilibrating movements in w were more than offset. Now consider a case in which the market is observed first at c', then at b' and finally at a. This is consistent with many market changes, two of which will be mentioned by way of illustration. First, both the demand and supply curves might be stable while the increase in wages restores equilibrium. Second, even though the demand curve is shifting to the right, the rate of increase in wages is fast enough to reduce the excess demand. When we observe either of these time sequences (a to b' to c' or c' to b' to a) we do not know what shifts in the curves have occurred but only that, in the first case, the shifts were such as to increase the disequilibrium in spite of equilibrating movements in w while, in the second case, any shifts that did occur either were not sufficient to offset the equilibrating changes in w or actually helped to remove the disequilibrium. If, to take a final example, the market is observed at b' over several successive periods, then we know that rightward shifts in demand and/or leftward shifts in supply were sufficient just to offset the equilibrating effects of changes in w, leaving excess demand constant.

It must be emphasised that knowledge of the shape of the adjustment function does not allow one to distinguish between *causes* of disequilibrium. Consider a market that is observed at a at time 0, at b' at $t=1$, at c' at $t=2$, at b' at $t = 3$, and finally at a at $t=4$. All we know is that there was an increasing disequilibrium associated with ever faster increases in w, but that after a while the disequilibrium lessened until, at $t=4$, it is completely eliminated. Now these observations are consistent with either a rightwards shift in the demand curve, first at an increasing rate and then at a decreasing rate, or with a leftwards shift in the supply curve, indicating first a rapid withdrawal of labour supplies and then a slower withdrawal.

The relation also raises the problem of the influence of unions, but, in fact, tells us very little about their influence on the market processes. There are a number of points to notice here. First, the observation of the postulated relation is quite consistent with changes in wages caused by union-induced shifts in the labour supply curve. For, as illustrated in the previous paragraph, shifts in the supply curve would give rise to observations lying on the adjustment function. Second, unions might influence the speed of the dynamic adjustment illustrated in Fig. 5. They might, for example, cause a faster increase of wages in response to excess demand and a slower fall in response to excess supply than would otherwise occur. In other words, they might shift the adjustment function to the shape illustrated by the dotted line in Fig. 5.[1] If a completely stable relation between $\dot{w}$ and u is observed over time, all that can be said is that, whatever is the influence of the union on the market, this influence has remained *relatively stable* over that time period.

We must now consider the effect of aggregating a number of markets each with the same relation between $\dot{w}$ and u in order to obtain a relation between $\dot{W}$ (the rate of change of a national index of wage rates) and U (the percentage of the whole labour force unemployed). The main problems can be illustrated in the case of two markets, α and β, *with identical reaction functions* of the sort illustrated in Fig. 7. We assume for simplicity in exposition that the labour force is divided equally between the two markets so that

$$U=\frac{u_\alpha+u_\beta}{2} \quad \text{and} \quad \dot{W}=\frac{\dot{w}_\alpha+\dot{w}_\beta}{2}.$$

Consider, first, what would happen if both markets always had identical levels of unemployment. Since the percentage of the labour force unemployed would be the same in both markets, the national index of percentage unemployment would be the same as the figure for the two markets ($u_\alpha=u_\beta=U$). Also, since both markets would be showing identical rates of change of money wage rates, the national index would show the same rate of change ($\dot{w}_\alpha=\dot{w}_\beta=\dot{W}$). If the level of unemployment then were allowed to vary in exactly the same way in both markets (so that $u_\alpha=u_\beta$ and $\dot{w}_\alpha=\dot{w}_\beta$), it follows that the observed relation between $\dot{W}$ and U would be identical with the relation between $\dot{w}$ and u in each of the individual markets.

Consider, second, what would be observed if aggregate unemployment were held constant at say $0a$ per cent. $\left(\frac{u_\alpha+u_\beta}{2}=0a\right)$, while the

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[1] It is worth noting that, if they were successful in shifting the reaction function, they could be affecting the distribution of the national product even though they were completely unable to shift either the demand or supply curve and thus were unable to affect the position of *equilibrium*. By increasing the *speed of adjustment* when there is excess demand and by slowing it down when there is excess supply, they would ensure that, over any finite time period, the average wage rate would be higher than it would otherwise be.

distribution of this unemployment were varied as between markets (say $u_\alpha < u_\beta$). Since the relation between $\dot{w}$ and u is non-linear to the left of the point a, wages will be increasing faster in the market with excess demand (α) than they will be falling in the market with excess supply (β). Therefore the national index of wage rates will be rising $\left(\dot{W} = \frac{\dot{w}_\alpha + \dot{w}_\beta}{2} > 0\right)$ in spite of the fact that the overall unemployment percentage remains unchanged at $0a$. Furthermore, as the distribution of U between the two markets is made less equal, $\dot{W}$ will take on larger and larger values since, when u_α is reduced by the same amount by which u_β is increased, $\dot{w}_\alpha$ will be increased by more than the amount by which $\dot{w}_\beta$ will be decreased.

Finally, consider what would happen if the two markets were kept in the same relation to each other (e.g. $u_\alpha = k \,.\, u_\beta$, where $k < 1$) while the total level of employment $\left(\frac{u_\alpha + u_\beta}{2} = U\right)$ were allowed to vary. As U varies, a relation between U and $\dot{W}$ will be traced out. We will call this curve A_m for *macro-adjustment curve* and distinguish it from the curves a_i for *individual market adjustment curves*. By the reasoning in the last paragraph, this relation between $\dot{W}$ and U will lie above the individual market adjustment curves. Now consider increasing the degree of inequality between two markets (i.e. reduce the value of k). Because of the non-linearity in the individual market relations between $\dot{w}$ and u, this will increase $\dot{w}_\alpha$ by more than it will reduce $\dot{w}_\beta$. Therefore $\dot{W}$ for the whole economy will be increased. It should be noted, however, that because of the linear relation to the right of a, this upward displacement will not occur if there is excess supply in both markets (u_α and $u_\beta > 0a$).

This analysis leads to a number of important conclusions about the relation between the individual adjustment functions (the a_i's) and the macro-curve (A_m). (1) The macro-function can never lie below the individual market functions.[1] (2) The macro-function will coincide with the individual (identical) a_i's only if there is an identical percentage of the labour force unemployed in each market at all levels of aggregate unemployment. (3) Whenever there is any degree of inequality in the distribution of unemployment combined with excess demand in at least one market ($u < 0a$ for some markets), the macro-observations will lie above the individual market curves for corresponding levels of unemployment. (4) The greater is the degree of inequality between markets, the further will the macro-observations be above the individual market curves, and thus the greater will be the degree of upward displacement of the observed macro-

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[1] If the a_i curves are not identical this conclusion reads: "The curve A_m can er lie below the "average" or typical curve a_i".

function.[1] The macro-function relating $\dot{W}$ and U will be *linear* only if there is excess supply in all markets (i.e. if *all* markets are in the range where the relation between $\dot{w}$ and u is linear). In all other cases it will be non-linear.[2]

These conclusions have a number of interesting real-world implications: (1) If one wishes to predict the rate of change of money wage rates ($\dot{W}$), it is necessary to know not only the level of unemployment but also *its distribution between the various markets of the economy.* It follows immediately that the observed macro-function need not be accepted as immutable even if the individual functions are. The macro-relation may be shifted by a policy designed to change the degree of inequality existing between the individual markets; if the distribution of U were made more even the macro-curve would shift downwards, thus increasing the downward flexibility of the overall wage level. (2) Because of the upward displacement of the macro-observations, the observed macro-relation between $\dot{W}$ and U will always tend to overstate the upward flexibility and to understate the downward flexibility of wage rates to be found in a typical individual market. (3) Thus, given non-linear a_i's, if a stable macro-relation between $\dot{W}$ and U *is* observed over a large number of cycles, it is implied that in both the upswing and the downswing roughly the same degree of inequality of unemployment has existed as between cycles.[3] (4) Finally, great caution must be exercised in trying to infer from a statistically fitted relation between $\dot{W}$ and U what would happen to wage rates if unemployment were held constant at any level for a long time. If unemployment were held constant, we would expect the degree of inequality in its distribution between markets to change substantially. We would thus expect the macro-adjustment function to shift.[4]

2. *The relation between $\dot{W}$ and $\dot{U}$*

Phillips noted that the actual values for $\dot{W}$ tended to be above the curve relating $\dot{W}$ to U when U was falling, and below the curve when

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[1] This conclusion can be upset only if the rate of response of wages to excess demand is slower than the rate of response to excess supply so that the a_i curves are kinked in the opposite way to the dotted function in Fig. 5.

[2] Thus the form of the function actually used (see equation 3) is to be understood as an approximation to the " true " curve which becomes linear (but with a negative slope) when there is excess supply in all markets. The difference between the $\frac{\partial^2 \dot{w}}{\partial u^2}=0$ of the theory and the $\frac{\partial^2 \dot{w}}{\partial u^2}>0$ of the fitted curve is slight over the range of u's studied, and the data is too crude to allow us to distinguish between the two.

[3] We would expect this to be true at least in some rough sense since certain sectors of the economy, e.g. the capital goods sectors, are typically hit much harder by fluctuations in the level of activity than are other sectors, e.g. the non-durable consumer goods sectors.

[4] It is an open question which way the curve would shift. It might be expected that a stable period would give time for the classical adjustment mechanism—movements of labour between markets and changes in relative prices—to reduce the degree of sectoral inequality. On the other hand, it might well be that cyclical fluctuations in employment aided the markets in adjusting to changes in demand and in techniques, and that the removal of these fluctuations would increase the average degree of inequality existing between markets.

that the demand for goods is rising, and that employers increase their demands for labour in the expectation of needing more in the future. As a result of this change, unemployment will be lower than it otherwise would have been (say 4 per cent.) and the rate of fall of wages will be less than it otherwise would have been (say -0.25 per cent.). There is, however, no loop; all that happens is that the point attained on the adjustment curve is different than would have been predicted solely on the basis of current demand for final goods; u is lower and $\dot{w}$ is higher than they otherwise would have been.

The difficulties encountered with these explanations in terms of a single market suggested that the origin of the loops might lie in the aggregation of the u's and the $\dot{w}$'s for a number of different labour markets each affected differently by fluctuations in the level of aggregate demand for final goods.[1]

Fig. 2 shows the observations of $\dot{W}$ and U for the twelve years 1868–79. The fitted relation between $\dot{W}$ and U lies in the middle of the observations, and this invites an interpretation of the " loops " as consisting of both positive and negative deviations from the relation $\dot{W}=f(U)$.[2] The theoretical argument of the present paper suggests, however, that this interpretation may be seriously misleading. The stable behavioural relation that we have postulated is the one between $\dot{w}$ and u in individual markets (see Fig. 7). The analysis of the previous section suggests that the macro-relation between $\dot{W}$ and U will *always be displaced upwards* from the individual market relation. The degree of upward displacement will be a function of the degree of inequality in the distribution of U between the various markets. This invites interpretation of the " loops " not as positive and negative deviations from a stable macro-relation between $\dot{W}$ and U, but as upward displacements from the stable single-market relations between $\dot{w}$ and u, the loops being produced by systematic variations in the degree of upward displacement.

In order to see how macro-observations of the type illustrated in Fig. 2 might arise, we may follow out the course of a hypothetical cycle in an economy with two imperfectly linked labour markets. Fig. 8 shows the (identical) relation between $\dot{w}$ and u in the two labour markets;[3] Arabic numerals refer to the positions at successive time periods in the two markets, while the crosses with Roman numerals show the corresponding aggregate observations that will be generated.

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[1] The hypothesis actually offered is by no means an untestable alibi. On the contrary, it leads to a number of testable hypotheses, other than the relation between $\dot{W}$ and U, that are of considerable interest. If the hypothesis stands up to these further tests it may be regarded as an interesting one. If it is refuted by these tests we shall learn from *the way in which it is refuted* much more about the conditions which must be fulfilled by its successor.

[2] Cf. Phillips, *loc. cit.*, p. 290.

[3] The assumption that the relations between $\dot{w}$ and u are identical in the two markets is relaxed later in the analysis.

Assume that the economy begins in a period of depression with heavy unemployment in one market (*a*) and lighter unemployment in the second market (*b*). The cross I indicates the percentage of the total labour force unemployed and the rate of change in a national

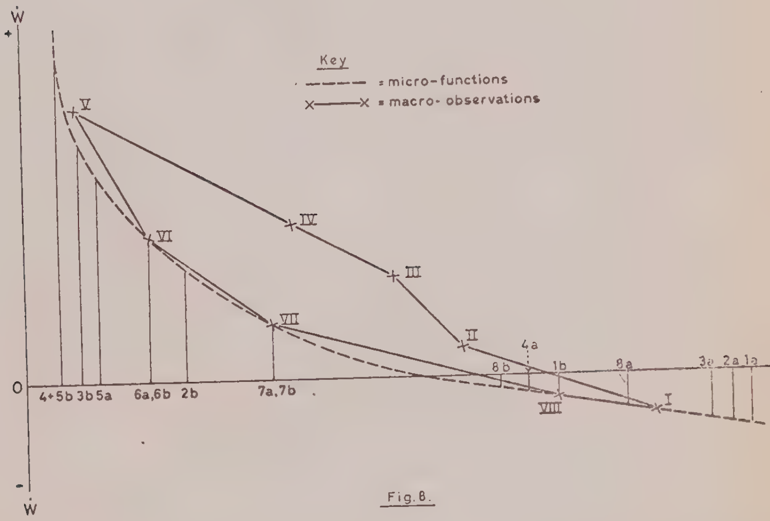

Fig. 8.

index of wage rates that will be observed. Now assume that a recovery starts, and that it is at first mainly centred in market *b*. As soon as excess demand occurs in market *b*, wage rates will begin to rise, although *U* for the whole economy is still high as a result of the heavy unemployment in market *a* (periods 2 and 3). As the excess demand in market *b* grows, the macro-observations will trace out a relation similar to that of the curve for market *b* but displaced to the right because of the influence of the heavy unemployment in market *a*. When market *a* begins to recover rapidly, *U* for the whole economy will fall rapidly but there will not be a further large increase in $\dot{W}$ until serious excess demand develops in market *a*. If both markets should reach the same level of excess demand, the macro-observations will lie on the micro-curve. Now assume a fall in activity in both markets. If U rises more or less uniformly in both markets, the observations of $\dot{W}$ in the downswing will lie near the micro-curves and, therefore, well below those for corresponding U's on the upswing. If the downswing comes before the same degree of excess demand is achieved in both markets, then the macro-observations for the downswing will lie above what they would be if the equality of excess demand had been achieved.

The " loops " can thus be accounted for on the hypothesis that the recovery affects different markets at different times while the fall in effective demand is, at least during the early stage of the recession, more evenly distributed.[1] Another way of making the same point is to say that the hypothesis requires that time lags are greater in the upswing than in the downswing. If a fall in demand in one market causes a fall in demand in other markets with a time lag of only a few months, then all markets will be observed to decline more or less together. If, on the other hand, there is a longer time lag before an increase in demand in one market is transmitted to other markets then all markets will not recover together and there will be a greater degree of sectoral inequality in unemployment in the early upswing than in the early downswing.[2]

This analysis points a general warning against the procedure of accepting statistically fitted relations without relating them to models of *market behaviour.* We have already seen that the data is consistent with the hypothesis that there is an association between $\dot{W}$ and $\dot{U}$ but it will be noted that, if a relation $\dot{W} = f(U)$ were fitted to the macro-observations of Fig. 8, the curve would go through the centre of the loop and thus be displaced upwards from the stable micro-adjustment functions. The observed macro-curve relating $\dot{W}$ and U goes through the centre of most of the " loops " and therefore gives the average relation between $\dot{W}$ and U, *given the degrees of inequality in excess demand that have in fact been experienced.* The macro-curve will thus be useful for prediction providing that the same sort of inter-market inequalities continue to occur. Great care must be taken in using the curve to predict what would happen if the level of U were held constant for some time for, if this were done, the degree of inter-market inequality in excess demand would be expected to change considerably.[3]

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[1] This would be true if, for example, the consumer-goods industries recovered first while the capital-goods sector did not recover until significant excess demand had developed in the consumer industries; while, on the other hand, when demand fell in one sector demand also fell in the other with less than a one year time lag.

[2] It may be objected that the assumption of identical relations between $\dot{w}$ and u greatly restricts the applicability of the model. This is not so. It has been shown in the text that varying degrees of sectoral inequalities in the distribution of unemployment (for which there is some empirical evidence) is a *sufficient* condition for the generation of the loops. If the loops are explained as a phenomena of aggregation then inequality in distribution is also a *necessary* condition. If for any level of U, u_{α} and u_{β} always bear the same relation to each other there will be unique relation between $\dot{W}$ and U for any relations: $\dot{w}_{\alpha} = f_1(u_{\alpha})$ and $\dot{w}_{\beta} = f_2(u_{\beta})$. If different reaction functions (about which we have little empirical evidence) were superimposed onto the model in the text the only difference would be a change in the *shape of the macro-loop.*

[3] This would be particularly important in the middle range of U values, where all experience has been of rapidly changing level of U, and less important at extreme values where U has more often been stable at least for two or three years at a time.

III

THE PERIOD 1919–1957

Phillips' scatter diagram relating $\dot{W}$ to U for the period 1919–57 is reproduced here as Fig. 9.[1] The diagram reveals very little relation between the level of unemployment and the rate of change of wages. Phillips argued, however, that there was evidence of a close relation between the two variables in certain periods. He also argued that, in the period following the second world war, the relation between $\dot{W}$ and U was substantially the same as it had been in the period 1861–1913. In the present section we will first consider the years 1919–58 as a single period and then consider the various sub-periods dealt with by Phillips.

1923–1957

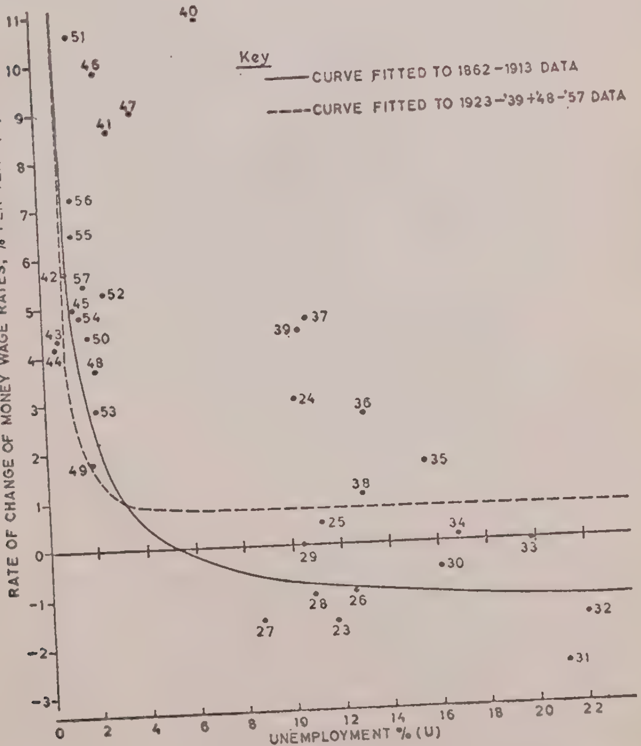

Fig. 9.

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[1] The figures for the years after 1945 have been amended in an attempt to make them comparable with the earlier figures. See section (5) below. The extreme values for 1919–22, are not included as the scale would have to be drastically reduced if they were. The $\dot{W}$ values are as follows: 1919 (+28·6), 1920 (+2·5), 1921 (−25·0), 1922 (−19·1).

When considering this period there are three major hypotheses that may be worth testing: (i) that the changes in money wage rates continue to be explained mainly by changes in U, $\dot{U}$, and $\dot{P}$; (ii) that the relative explanatory power of these three independent variables is unchanged; and (iii) that the exact relation between $\dot{W}$, U, $\dot{U}$, and $\dot{P}$ is unchanged so that equation (10) which was fitted to the years 1862–1913, predicts accurately the experience of this period. The first hypothesis is in fact borne out by the data but the second and third are refuted.[1]

1. *The years 1920–39 and 1947–57*[2]

The curvilinear relation between $\dot{W}$ and U described by equation (3) was first fitted to the data for this period. R^2 for this relation is 0.28 which indicates that only a low proportion of the variance in $\dot{W}$ can be accounted for by variations in U. Next the variables $\dot{U}$ and $\dot{P}$ were added (i.e., equation 9 was fitted to this data) and the R^2 increased to 0.88. The squared partial correlation coefficients for this relation are $U=0.06$, $\dot{U}=0.05$, and $\dot{P}=0.83$ which indicate that $\dot{P}$ is the most important of the three explanatory variables while U and $\dot{U}$ add practically nothing to the explanation of $\dot{W}$. Even more startling than the high partial correlation between $\dot{W}$ and $\dot{P}$ is the magnitude of the regression coefficient for $\dot{P}$. This has a value of 1.28, indicating that on the experience of the whole period a 1 per cent. rise in prices is associated with a rise in wages of more than 1 per cent.

There are, however, a few very extreme values for $\dot{W}$ in this period and their existence poses some serious problems. The variance in $\dot{W}$ over the period under consideration is 47.2 while, if the four years 1920, 1921, 1922 and 1947 are eliminated, the variance drops to 10.2.[3] Thus 13 per cent. of the years account for 78 per cent. of the variance in $\dot{W}$, and any explanatory variable which accounts for $\dot{W}$ in these four years will necessarily produce a high R^2 for the whole period

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[1] The model used throughout the present paper is the simplest sort of "single-equation model". This report is a part of a larger study in which simultaneous relations will be used. The single-equation model is probably justified as a first approximation especially where it is desired to try alternative explanatory variables, alternate specification of the lags, and alternative functional forms. The single-equation model does, however, introduce some serious biases into the estimates. The most serious error is likely to be on $\dot{P}$ which is the main variable affected by other parts of the system. It is easily shown, however, that the regression coefficient of $\dot{P}$ is biased upwards. Calculations taking what appear to be reasonable specification of the dependence of prices on wages, suggest that the bias may be of the order of 0·2 to 0·3 (so that the subsequent estimate of 0·69 for the coefficient of $\dot{P}$ should probably be more like 0·40 to 0·50).

[2] The periods of the two world wars plus the first post-war year (1914–19 and 1940–46) were omitted on the argument that a period of heavy war-time controls is not an appropriate period from which to infer the relations being studied.

[3] It is probably reasonable enough to eliminate 1920 and 1947 on the argument of the previous footnote. One is less certain about the years 1921 and 1922.

irrespective of its ability to account for variations in $\dot{W}$ over the remaining years. It is also true that the regression coefficient associated with this variable will mainly reflect the relation between it and $\dot{W}$ over these four years. There is therefore good reason to mistrust the regression coefficients calculated from a series containing a few such very extreme values which must dominate the whole estimation procedure. For this reason the four extreme years were dropped from the series and the various relations recalculated.

2. *The years 1923–39 and 1948–57*

The curve fitted to this period was of the form:[1]

$$\dot{W}=a+bU^{-1}+cU^{-4}+d\dot{U}+e\dot{P} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (12)$$

which, when fitted to the data, gives

$$\dot{W}=+0.74+0.43U^{-1}+11.18U^{-4}+0.038\dot{U}+0.69\dot{P} \quad \ldots (13)$$

R^2 for this relation is 0.91, while the squared partial correlation coefficients are $U=0.38$, $\dot{U}=0.30$ and $\dot{P}=0.76$.[2]

There are a number of interesting things to note about these results.

(i) *The General Relation*: A very high proportion of the variance in $\dot{W}$ can be associated with these variables.[3] Thus the hypothesis that about the same proportion of the variance in $\dot{W}$ can be associated with U, $\dot{U}$ and $\dot{P}$ as in the earlier period is consistent with the facts.

(ii) *The Variable $\dot{P}$*: The importance of $\dot{P}$ as an explanatory variable has greatly increased compared with the pre-war period (a squared partial correlation coefficient of 0.76 compared to 0.17). The regression coefficient for $\dot{P}$ has also increased greatly (0.69 compared to 0.21). This indicates a substantial movement in the direction of a one-one relation between changes in prices and changes in wages. This is an extremely interesting change. The face-value interpretation is that the demand and supply model of Section II accounts for less of the variations in wage rates in the twentieth century than it did in the nineteenth, while more of the twentieth-century variations can be explained in terms of wages "chasing" prices or of prices chasing wages.

(iii) *The Variable U*: The fitted relation between $\dot{W}$ and U has changed substantially compared with the earlier period. The curve

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[1] The curvature became much sharper in this period than it was in the nineteenth century. If Phillips' curve (2) were fitted to the data the coefficient γ would have been less than minus two. Thus the fixed coefficients on U had to be changed. The relationship described by equation (12) is the same as that described by (10) only the curvature is more marked.

[2] The standard error of estimate for $\dot{W}$ is $0\cdot97$, standard errors for the regression coefficients are: $b=2\cdot10$, $c=6\cdot00$, $d=\cdot012$, $e=0\cdot08$. There is no evidence of significant auto correlation of the residuals for lags of one to three periods at the 5 per cent. probability level. Correlation of the "independent variables" is as follows:

$$r^2\,(U, U)=0\cdot003;\quad r^2\,(U, \dot{P})=0\cdot47;\quad r^2\,(\dot{U}, \dot{P})=0\cdot09$$

[3] Corrected for degrees of freedom, the R^2 is $0\cdot89$.

relating these variables has pivoted about the 3 per cent unemployment level (see Fig. 9). The new curve lies above the old one for levels of U greater than 3 per cent. and below the old one for levels of U less than 3.[1] This indicates that the post–1922 experience was of less flexibility of wages in response to excess demand, whether positive or negative, than occurred in the pre–1914 period. It will also be noted that the asymptote in equation (13) is positive. This indicates that, on the *average experience* of the 1920's and the 1930's, high levels of unemployment are not in themselves associated with a reduction in wages. Here again the theory of Section II must be recalled and the qualification " given the degree of sectoral inequality in unemployment that then existed " be emphasised.

(iv) *The Variable* $\dot{U}$: Finally, we must note the interesting changes in the relation between $\dot{W}$ and $\dot{U}$. Comparing equations (13) and (10), we see that the regression coefficient for $\dot{U}$ has changed signs. This reveals that, on the average experience of the post-1922 period, other things being equal, times of falling unemployment were associated with lower $\dot{W}$'s than were times of rising unemployment. It would appear then that Phillips' loops have changed directions. Before considering a theoretical explanation of this experience, it is necessary to check the relation between $\dot{W}$ and $\dot{U}$ in various sub-periods in order to determine exactly what it is that has to be explained. The data were broken up into three time periods and the following equation was fitted to each period[2]

$$\dot{W}=a+bU+c\dot{U}+d\dot{P} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (14)$$

The coefficients for $\dot{U}$ were as follows:

$$1923\text{–}29=+1.91, \quad 1929\text{–}39=-6.25, \quad 1948\text{–}57=+3.28$$

Thus we see that, taken period by period, the experience of the 1930's agreed with that of the nineteenth century while that of the 1920's and 1950's did not.

We must now ask if this experience can be explained by our theory. Feeding this data into the theory, we obtain the testable predictions that in the 1930's the upswing was associated with increasing degrees of sectoral inequalities in unemployment as some markets recovered very much more rapidly than did others. On the other hand, in the 1920's and the 1950's, downswings in activity were accompanied by increases in sectoral inequalities, while upswings were associated with decreases. Here again the theory accounts for the observations by producing hypotheses that are clearly testable. These tests, which are being conducted, are beyond the scope of the present paper.

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[1] When the curve is fitted without a cost of living variable [$\dot{W}=f(U, \dot{U})$], it shifts upwards from its pre-1914 position over its whole range, but when $\dot{P}$ is added the curve is found to pivot as described in the text.

[2] The range of variations in U within each of the three periods is such that a linear approximation to the relation between $\dot{W}$ and U is quite adequate. A similar disaggregation for the nineteenth century is summarised in Table 1.

These considerations point to the rejection of hypotheses (ii) and (iii) listed above. Hypothesis (ii), that the variables have had the same relative importance in explaining $\dot{W}$ in the periods before and since the first world war is refuted by the fact that the partial correlation coefficients relating $\dot{W}$ to each of the independent variables have changed considerably. Hypothesis (iii) is refuted by the fact that the regression coefficients have changed markedly.

In the following sections the period under consideration is broken up into three sub-periods. By comparing the predictions of equation (10) fitted to the period 1862–1913 with those of equation (13) fitted to the present period, we seek to determine how and when these relationships have shifted.

3. *The period 1920–29*

In the years 1920 and 1921, very large decreases in both the cost of living and money wage rates were experienced. When $\dot{W}$ for these years is predicted from equation (13) the errors are extremely large (a residual of 16.8 per cent. for 1921 and of 9.8 per cent. for 1922). This shows that the relation describing the remainder of the period (equation 13) is not a good description of these two years. This is mainly because the relation between $\dot{W}$ and $\dot{P}$ seems to be stronger in these two years than it is over the rest of the period.[1] In the years 1925 to 1929 the government attempted to check aggregate demand in order to reduce the price level. Unemployment stayed at about 10 per cent., while wage rates fell on the average less than 1 per cent. per year. Phillips makes the point that the results of this experiment could have been predicted quite accurately on the basis of the experience for 1861–1913. The average annual reduction in wage rates that in fact occurred over the five years 1925–29 was 0.60 per cent. The prediction for the annual average reduction made from the equation fitted to the 1862–1913 experience (equation 10) is 1.00 per cent. We must conclude, therefore, that there was no reason to be surprised at the very slow reduction in wage rates that actually occurred, and that the experience of the late 1920's seems to provide little evidence of diminished downward flexibility of wage rates. The measurements give strong support to Phillips' statement (*loc. cit.*, p. 295) that: "The actual results obtained, given the levels of unemployment which were held, could have been predicted fairly accurately from a study of the pre-war data, if anyone had felt inclined to carry out the necessary analysis."

4. *The period 1930–39*

The equation fitted to the pre–1914 data consistently underestimates the changes in wage rates over this period. In only three years, 1936,

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[1] When $\dot{W}$ is estimated from the equation fitted to the years 1920–39 and 1947–57, the residuals are only 6·8 per cent. for 1921 and 1·8 per cent. for 1922. This shows how much the estimated relation is influenced by these extreme years.

1937 and 1939, does equation (10) not predict a fall in wage rates. The average annual predicted fall over the whole period is 0.54 per cent., while the average annual error $\Sigma/R_{10}/ : n$ was 1.67 per cent. In fact, money wage rates rose from 1934 onwards and the average annual change in wage rates over the ten-year period was +0.99 per cent. Equation (13), on the other hand, predicts this result quite accurately at 0.89 per cent. Some of the errors in individual years are, however, quite large and the average annual error over the period is 0.74 per cent. We must conclude that there is evidence that $\dot{W}$ increased faster in the 1930's than in the pre–1914 period for comparable levels of U, $\dot{U}$, and $\dot{P}$.

It should also be noted that wages rose from 1934 onwards in spite of very high levels of unemployment (never less than 10 per cent. over the entire period). There are two probable causes of this experience. First, if we accept the results of the statistical analysis as showing an increased response of wages to changes in the cost of living, much of the rise in wages in the 1930's can be explained as a response to such changes (the average annual increase in the cost of living between 1934 and 1939 was 3.15 per cent.). If, however, the increase in wages due to cost of living changes is estimated from equation (13) and this amount deducted from the actual increase, the result is still positive.[1] Something further, then, is required to explain that part of the increase in wage rates not associated with $\dot{P}$. A second reason may be found in the degree of sectoral inequality in unemployment rates. The theory produces the testable hypothesis that from 1935 onwards there was sufficient excess demand in some markets to cause an increase in the national index of wage rates in spite of extremely large excess supplies in other markets.

effect of

5. *The period 1948–57*[2]

The following table shows the observed and the predicted changes in wage rates over the period.[3]

It will be noted that the average annual increase predicted from equation (13) agrees very closely with the observed annual average, while the average predicted from equation (10) considerably underestimates the observed figure. Considering the predictions from

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[1] These corrected figures for $\dot{W}$ 1934 to 1939 are:
−0·62, +0·22, +0·13, +2·68, +0·13, −0·98.

[2] A detailed study of this period is to be found in L. A. Dicks-Mireaux and J. C. R. Dow, " The Determinants of Wage Inflation: United Kingdom, 1946–56 ", *The Journal of the Royal Statistical Society*, 1959. These authors obtain a coefficient of wages on prices of approximately 0·50.

[3] The figures have been changed to make them comparable with the earlier ones. The increase in coverage after the second war has been mainly in groups with very low unemployment percentages. Thus the post-war figures are not comparable with the pre-war ones. Mr. Routh (*loc. cit.*, p. 367) estimates that the figures for U must be raised by a minimum of 12½ per cent. in order to make them comparable with the earlier figures. In the present study the figure has been increased by 20 per cent. The most accurate adjustment probably lies somewhere between these two figures.

TABLE 2

Year	Observed $\dot{W}$	Estimated $\dot{W}$ equation 13	Error	Estimated $\dot{W}$ equation 10	Error
1948 . .	3·73	2·89	0·85	4·28	–0·54
1949 . .	1·82	3·58	–1·76	3·51	–1·69
1950 . .	4·40	5·71	–1·31	4·29	0·11
1951 . .	10·61	9·95	0·66	5·57	5·04
1952 . .	5·28	5·49	–0·21	2·76	2·52
1953 . .	2·90	2·49	0·41	3·37	–0·47
1954 . .	4·88	3·76	1·12	4·53	0·35
1955 . .	6·58	7·32	–0·74	5·92	0·66
1956 . .	7·31	6·87	0·44	4·80	2·51
1957 . .	5·50	5·19	0·31	3·09	2·41
Mean . .	5·30	5·33	0·78[1]	4·21	1·63[1]

equation (13), the large errors occur in the years 1949, 1950 and 1954. 1949–50 were the years of wage restraint, and the large errors shown in Table 2 indicate that over the two years wages rose much less than would have been expected from the experience of the rest of the period 1923–57. These large errors provide a measure of the effectiveness of the wage policy. In 1954, on the other hand, the increase in wage rates was more than 1 per cent. in excess of the value predicted from equation (13). We must conclude, therefore, that, except for 1949 and 1950, there is evidence of a more rapid increase in wages in response to demand and prices in the period since the second world war than in the period prior to the first world war.

Phillips used his curve relating $\dot{W}$ to U to predict the level of unemployment that would be compatible with stable prices and a 2 per cent annual increase in productivity (a little under 2½ per cent. according to Phillips). There are at least three very serious problems involved here. (i) The estimated value can be shifted a great deal by fitting curves of different types, by including additional variables and by excluding particular years. *Thus, although it might be held with a high degree of confidence that a significant and very interesting relation had been discovered, a very low degree of confidence might be attached at this stage to a particular estimate of the parameters.*[2] (ii) The theory outlined in Section II suggests that the fitted relation may not be a very good guide to the relation between $\dot{W}$ and U if U were to

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[1] Ignoring signs.

[2] Mr. Routh in the article already cited has constructed some alternative series to the ones used by Phillips and has done some alternative correlations. He concludes (p. 314): " I have shown that there are other equations, in some ways more valid, that would give different results." To my mind, the remarkable thing is not that Mr. Routh is able to get different results, but that the differences are so slight for all the possible variations that he suggests (see, for example, his Diagram I, p. 311). He also appears to be rather uncritical in assessing the significance of his possible variations. For example, he suggests a possible alternative to the series used by Phillips for 1948–57 and concludes " The points in Professor Phillips' Fig. ii, if row 2 (Routh's series) were substituted for row 1 (Phillips' series), would

remain substantially unchanged for a long time. (iii) A satisfactory theoretical explanation (together with independent tests) would be needed of the high correlation between $\dot{W}$ and $\dot{P}$. Until more is known about the causal links between $\dot{W}$ and $\dot{P}$ it is very dangerous to argue as if either of these variables were independent of the other.[1]

The analysis given in this paper has considerable bearing on the controversy about the causes of inflation. There are a number of points to be noted here. Phillips clearly considered a high correlation between $\dot{W}$ and U as evidence in favour of a demand-pull as against a cost-push hypothesis. This is not the occasion to state these hypotheses in sufficient detail to make them testable. However, the theory outlined in Section II suggests that there are versions of the cost-push hypothesis which are compatible with this relation.[2] The present study does, however, seem to refute the extreme version of the cost-push *spiral* which envisages an unstable situation in which wages and prices chase each other in a non-covergent cycle. This theory predicts a one-one relation between changes in prices and changes in wages, and the present coefficient of 0.69 would, if correct, refute the theory. On the other hand, it must be noted that the considerable increase in the coefficient attached to $\dot{P}$ indicates a very much closer association between changes in prices and changes in wages after the first world war than before it. Only a very much more detailed analysis than that conducted here could attempt to sort out the direction of the causation between $\dot{P}$ and $\dot{W}$. The analysis so far conducted is, however, not inconsistent with the hypothesis that there is a strong feed-back from price changes to wage changes with a great deal *but not all* of the rise in wages being attributed to wages chasing prices.

In my opinion it would be a serious mistake to try to judge between cost-push and demand-pull hypotheses solely, or even mainly, on the basis of the present paper although the material presented here is relevant evidence. The conclusions of this analysis would seem to be much more important for economic theory than for *immediate* policy issues. At this stage the numerical values of any of the parameters is not so important; what is important is the possibility of measuring and testing the type of dynamic relation used here, and of building up a theory that will, as ours already has done, suggest further hypotheses, the testing of which will in turn suggest further improvements in the theory.

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no longer ' lie closely along a smooth curve which coincides almost exactly with the curve fitted to the 1861–1913 data ' (p. 306)." This is just not correct. Consider the deviations of the two series from Phillips' curve. The standard deviation of the residuals for Routh's series are 1·6 and for Phillips 1·9, while the mean deviations are 1·4 for Routh and 1·1 for Phillips. We must conclude therefore that there is very little between the two series as far as lying on the curve is concerned.

[1] When policy decisions must be made they have, of course, to be based on the best evidence available at the moment. A premature application to policy can, however, easily discredit a hypothesis that is potentially very fruitful.

[2] See pp. 16 and 17.

The Firm's Demand for Intermediate Products[1]

By J. R. Gould

An important problem facing the firm is which intermediate products to manufacture for itself and which to purchase from other firms through the market—in short, whether *to make* or *to buy in*. This problem has received relatively little attention from economists, perhaps, as G. J. Stigler suggests, because " . . . they have generally treated as a (technological ?) datum the problem of what the firm does—what governs its range of activities or functions ".[2]

The complexity of the problem and some of the most important considerations the firm has to take into account are discussed by R. S. Edwards and H. Townsend. In the course of summing up their discussion they say: " Comparing the costs of buying and making is very difficult. The risks of contracts not being honoured or deliveries being delayed, the risks of capacity being set up and business afterwards declining, means inevitably that there is room for differences of opinion in cost estimates ".[3] Nevertheless, it is useful to abstract from the dynamic problems of risk and of the growth of the firm, firstly, because of the insights which may be gained from simplification, and secondly, because in some cases the actual situation may be in close accord with the static assumptions.

The first step is to examine the equilibrium conditions for a firm faced with the choice of either making or buying in. It will be pointed out that the received analysis is faulty because it examines the problem solely from the point of view of the costs of the firm and neglects the revenue function. In particular, section I of this paper will seek to establish by quotation that the current view seems to be that it will not pay the firm to buy in when the market price of the intermediate product is greater than both the average and marginal costs of production within the firm. Contrary to this view, and somewhat paradoxically, it will be shown in section III that in some circumstances it will pay the firm to buy in at some market prices which are *above* both the average and marginal costs of production within the firm.

In section IV some criticisms of the analyses quoted in section I will be offered, together with a brief discussion of the firm's demand for intermediate products. Sections V and VI will discuss some implications of the theorems presented in section III.

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[1] The author is indebted to Mr. G. C. Archibald, Dr. R. G. Lipsey and Mr. M. H. Peston for much helpful comment and criticism.

[2] George J. Stigler, " The Division of Labour is Limited by the Extent of the Market ", *The Journal of Political Economy*, Vol. LIX (1951), p. 185.

[3] R. S. Edwards and H. Townsend, *Business Enterprise: Its Growth and Organisation*, 1958, p. 254.

I

The first example of treatments of the problem by economists is taken from an article addressed to accountants by R. H. Coase in which he prescribes the use of the concept of marginal cost.

> As an example of the use of the marginal cost concept, I examined the case of an electricity supply undertaking which owned a coal mine and which had to determine how much to produce in its own mine. My conclusion was that the most profitable policy for this undertaking would be to produce coal in its own mine so long as total avoidable costs were covered and marginal cost was not greater than the cost of purchasing the coal on the open market. It would seem that there are two questions which the management would have constantly to be asking. First of all, it would have to consider whether the expenses that it would save by not producing anything (the avoidable cost) are greater or less than the amount it would have to pay for the same quantity. The other question is whether the cost of producing one more unit is greater or less than the expense of buying it.[1]

S. Weintraub discusses integration in the course of his treatment of the multi-product firm and reaches the same conclusions as Professor Coase.

> Profitwise, it will be prudent for the firm to integrate backwards whenever the marginal cost of performing an additional process is below the market price for the same intermediate factor. If extra equipment is indispensable for the new task, the full condition is that $P = MC > AC$, where P is the market price for the material, MC is the additional cost of producing it within the firm, and AC is the full average cost of producing it within the firm. The inclusion of AC in the equilibrium condition is dictated by the fact that if MC lies below AC it would be foolhardy for the firm to incur the separate fixed costs to widen its productive sphere.[2]

The third example is from Professor Stigler's discussion of the effect of the size of the market on vertical disintegration.

> At a given time these functions (subject to increasing returns) may be too small to support a specialised firm or firms. . . . The firm must then perform these functions for itself.
>
> But, with the expansion of the industry, the magnitude of the function subject to increasing returns may become sufficient to permit a firm to specialise in performing it. . . . This new firm will be a monopoly, but it will be confronted by elastic demands: it cannot charge a price for the process higher than the average cost of the process to the firms which are abandoning it.[3]

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[1] R. H. Coase, " Business Organisation and the Accountant ", *The Accountant*, Oct. 1 to Dec. 17, 1938. (Reprinted in David Solomons (ed.), *Studies in Costing*, 1950, pp. 105–58.)

[2] S. Weintraub, *Price Theory*, 1949, p. 304. Taken literally, the last sentence would mean that a firm should never perform a process subject to decreasing costs, no matter how high the market price of the process. In its context, I take it to mean that when $AC > MC$ the equilibrium condition is $P = AC$.

[3] Stigler, *loc. cit.*, p. 188.

The examples given above indicate that the received view is that a firm making an intermediate product subject to decreasing average costs will continue to do so as long as the market price of the intermediate product is greater than its average cost of production within the firm. When the market price falls below the average cost, the firm will cease operating the process altogether and buy in all its requirements. If the firm is making all its requirements of the intermediate product subject to increasing average costs, the analyses of Professors Coase and Weintraub would suggest that it would not pay to buy in if the price were higher than its present marginal cost.

Both these conclusions are inconsistent with profit maximisation as the firm's objective. When the intermediate product is being made subject to decreasing average costs there will be a range of market prices for it *above* the firm's present average cost in which it will pay to buy in all requirements. When the firm makes all it needs of the intermediate product subject to increasing average costs, there will sometimes be a range of market prices above the firm's present marginal cost in which the firm would find it profitable to buy in supplies. These results will be demonstrated in section III.

II

It will be convenient for purposes of exposition to consider a firm which up to now has been making all its requirements of the intermediate product in question. It now has the opportunity of buying in. Given the cost and revenue functions of the firm, the problem is to find the maximum price it would be prepared to pay to an outside supplier.

Of course, a firm which has been buying all its requirements will view the situation rather differently, since it may have to invest in some fixed equipment if it is to make the intermediate product itself. However, providing the concept of opportunity cost is used, the problems of whether to buy when already making, and whether to make when already buying, are symmetrical. The formal analysis of one case will serve for both.

It is assumed that the firm manufactures a single final product which entails several processes. The output of the intermediate product under consideration (Y) is directly proportional to the output of the final product, that is, the technical coefficient of production of Y is fixed. The cost of Y is independent of the rate of output of the other intermediate products; similarly, the rate of output of Y does not affect the cost of the other intermediate products.[1]

The net marginal revenue curve (NMR) of Y is obtained by deducting from the marginal revenue curve of the final product the marginal cost curves of the intermediate products other than Y. The firm will be maximising profits at the output which equates the marginal cost of *Y* with *NMR*. It will be assumed that the *NMR* curve is shaped like an inverted *U*.[1]

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[1] The model used here is an adaptation of the one used by Professor Stigler in his article " The Division of Labour is Limited by the Extent of the Market ".

Since both the *NMR* curve and the *AC* curve of *Y* may be rising or falling, the firm may be producing under four possible conditions:

(i) The *NMR* curve rising and the *AC* curve of *Y* falling. It can be shown that in these conditions there will either be no position of stable equilibrium or, if there is one (i.e. the marginal cost curve cuts the *NMR* curve from below), the firm will be making losses. Thus this case can be disregarded.

(ii) The *NMR* curve falling and the *AC* curve of *Y* rising. This is the case which Professors Coase and Weintraub must have had in mind in their consideration of the problem and for which their conclusions are correct. The case is further discussed in section IV of this paper.

(iii) The *NMR* curve rising and the *AC* curve of *Y* rising.

(iv) The *NMR* curve falling and the *AC* curve of *Y* falling. Cases (iii) and (iv) will be considered in section III.

III

In Fig. 1 the case is considered of the firm producing *Y* on a downward sloping portion of the *NMR* curve under conditions of decreasing average costs. The cost curves are not shown.

It is assumed that the firm is maximising profit at an output of *OC*; at this output *NMR* equals the marginal cost of *Y*, i.e. equals *MC*.

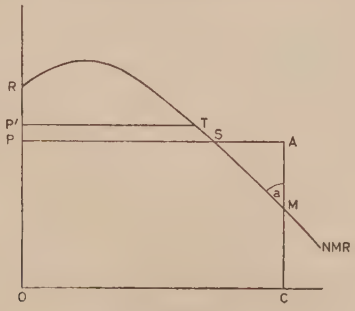

FIGURE 1

Let *AC* be the average cost of output *OC*. Since $AC > MC$ average cost must be falling (marginal costs may be rising or falling—it makes no difference to the argument). By deducting from total net revenue $RMCO$ the total cost of Y, $PACO$, the profit is found to be $RSP—SAM$.

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[1] The slope of the *NMR* curve at any output depends on the slopes at that output of (*a*) the marginal revenue curve of the final product, and (*b*) the aggregate marginal cost curve of the processes other than *Y*. If (*b*) slopes downwards *more* steeply than (*a*) the *NMR* curve will slope upwards. If (*b*) slopes downwards *less* steeply than (*a*), or (*b*) slopes upwards, the *NMR* curve slopes downwards. Since marginal cost curves are often represented as sloping downwards for small outputs and then sloping upwards for larger outputs, it will be as well to consider the possibility that the *NMR* curve slopes upwards for small outputs.

Suppose that another firm now offers to supply Y at a price $OP(=AC)$ per unit for any quantity. PA becomes both the average and marginal cost curves for Y, and the new profit maximising output equals PS giving a profit of RSP. Thus at a market price equal to the average cost of making Y within the firm, it will pay to buy in all requirements, the extra profit being SAM.

It is apparent that there will be some price (OP') greater than OP, below which it will pay the firm to buy in all supplies of Y, and above which Y should be made within the firm. In Fig. I, OP' is found by drawing $P'T$ so that $P'TSP=SAM$. At a market price of OP', the firm will be indifferent between buying in or making, since the profit in each case will be the same, namely RTP'. Thus it has been shown that it will pay the firm making an intermediate product subject to decreasing average costs to buy in all requirements at some market prices of the intermediate product *above* the present average cost of production within the firm.

The second case to be considered is that in which the firm is producing Y on an upward sloping portion of the NMR curve under conditions of increasing average costs.

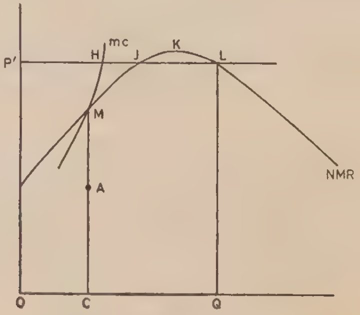

FIGURE 2

In Fig. 2 the marginal cost curve of Y is represented by mc. The firm is maximising profits where $mc=NMR$, i.e. at output OC, and marginal cost MC.

The opportunity now arises of buying in Y at a price of OP' ($>MC$). The marginal cost curve of process Y now becomes mc at outputs to the left of H and HL to the right of H. This marginal cost curve cuts the NMR curve at M, J and L. J is, of course, not a maximum since the marginal cost curve cuts the NMR curve from above. M and L are both positions of stable equilibrium.

Moving from output OC to output OQ increases revenue by the area $CMKLQ$; increases costs by $CMHLQ$; and thus increases profit by the difference between these two areas, namely $JKL—MHJ$. There will be some value of OP (say OP') greater than MC such that $JKL = MHJ$ and the profit from producing output OC equals the profit from OQ. OP' is the maximum price the firm would pay an outside supplier for Y. In this case the firm producing all its requirements of an intermediate product would be prepared to pay a price to an outside supplier greater than its present marginal cost of production within the firm.

IV

The analyses quoted in section I can be criticised on the ground that, although they deal with profit-maximising firms, the revenue function is ignored. Indeed, it would seem that the problem has been treated as one of minimising the cost of some particular output—at any rate, this is a problem to which the answers given are correct ones. It is true that a profit-maximising firm will produce its output at the minimum possible cost. However, it is obviously not true that the firm first decides on its output and then finds the most efficient way of producing that output. The sequence is that it first finds the minimum costs of each output (the conventional cost curve) and then, with the aid of the revenue function, decides on its profit-maximising output.[1]

For the special case of the firm which is already buying in, and whose average costs of making within the firm would be rising at outputs below its present one, Professors Coase and Weintraub give correct answers to the question: How much should the firm make for itself? For this case neglect of the revenue function does not lead to error. However, when the intermediate product is being made subject to decreasing average costs, the wrong answer is given. There does not seem any *a priori* reason why the case of increasing should be more important than that of decreasing average costs; indeed, Professor Chamberlin's tangency solution for the long-run equilibrium of the firm in monopolistic competition might suggest the reverse. If the NMR curve is upward sloping the $P = MC$ rule breaks down if the firm is currently making but not buying in, even if the AC curve is rising.

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[1] The distinction between, for example, Professor Coase's approach to the problem of making or buying in, which aimed at minimising the cost of a particular output, and the analysis of section III, which aimed at maximum profit, is of a kind which operational research workers have to make in selecting appropriate criteria against which to measure possible solutions to business problems. (See Charles Hitch, " Sub-optimization in Operations Problems ", *Journal of the Operations Research Society of America*, Vol. I (1953), p. 87.)

If a firm can be told of a method by which it can reduce the cost of a particular output it has decided to make, quite clearly it can make more profit. However, its position can be further improved if the operational research worker can take into account the possibility of changing the output. For this purpose some estimate of the revenue function is needed.

In many cases the revenue function will be much more difficult to estimate than the cost function. Where it is considered impossible to arrive at a reliable estimate of the revenue function, the operational research worker may be forced to fall back on the " lower level " criterion of cost minimisation. Even in this case he will be able to say in which direction output should change to increase profit. The direction of the changes in output and price are indicated in section VI of this paper.

Complementary to the question of how much a firm should make is that of how much a firm should buy in. A full analysis of the firm's demand curve for intermediate products is beyond the scope of this paper; it would take into account the possibilities (*a*) that the rate of output of one intermediate product might affect the cost of another, and (*b*) that there might be relations of substitution or complementarity between processes. However, it may be suggested that the analysis would be similar to that of the demand for primary factors, e.g. labour, in that the demand for the intermediate product would be derived from the demand for the final product and the costs of the other processes; it would differ from the analysis of the demand for labour because account would have to be taken of the option the firm has of producing the intermediate product itself.

Within the framework of the assumptions outlined in section II, a diagrammatic representation of the firm's demand curve for outside supplies of an intermediate product can be given. This demand curve takes different shapes depending on whether the firm is making the intermediate product subject to increasing or decreasing average costs and on the shape of the *NMR* curve.

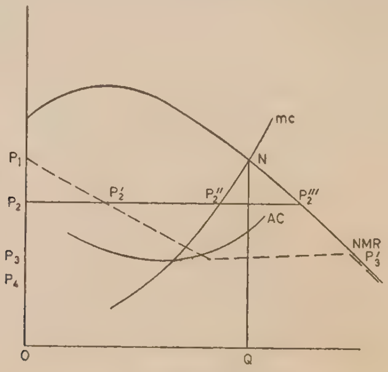

FIGURE 3

In Fig. 3 the firm is operating the process subject to increasing average costs at an output OQ. The firm's demand curve for the process is represented by the broken line. Above a price OP_1 $(=NQ)$, the firm's demand is nil. At a price OP_2, above the minimum average cost of production within the firm but below OP_1, the firm will produce an amount $P_2\,P_2''$ within the firm and buy in a further amount $P_2''\,P_2'''$. P'_2, then, is a point on the demand curve with $P_2\,P_2'=P_2''\,P_2'''$. At a price P_3, equal to the minimum average cost of production within the firm, the firm will be indifferent between buying in all its requirements, $P_3\,P_3'$, and making up to the point of minimum average cost and buying in the balance. At any price P_4, below P_3, the firm's demand is given by the corresponding point on the *NMR* curve.

In a similar manner the firm's demand curve for intermediate products under different assumptions about the shapes of the cost and *NMR* curves can be derived. Two points about these demand curves are worthy of note. Firstly, they are " kinked " in one or more places, so that the marginal revenue curves derived from them will have discontinuities. This suggests possibilities of situations in which price might be insensitive to changes in costs.

Second, in the analysis of some problems, the demand curve for intermediate products would be used in conjunction with the cost curves of outside suppliers. It may very well be the case that a change which affects the cost curves of the outside suppliers will also affect the demand curve. For example, say the price of a factor used in the manufacture of the intermediate product falls and this factor is used both by the suppliers of the intermediate product and its users if they make it themselves. Both the cost curves of the suppliers and the demand curve will fall—the demand curve because the costs of the process enter into its derivation.

V

The idea that there is a connection between the degree of vertical integration and the degree of monopoly has been arrived at from two main lines of argument. The first is that the monopolist may integrate vertically in order to hinder entry; the second comes from the analysis of bilateral and successive monopoly which indicates that the sum of the profits of two monopolists dealing with each other at arm's length may be less than could be got by combining the two firms.[1] The theorems of section III provide another line of approach to the relation between monopoly and the degree of vertical integration.

The case where the intermediate product is being produced subject to decreasing average costs is taken first. The price which a firm would be prepared to pay to an outside supplier can be shown to be a decreasing function of the slope of the *NMR* curve.[2] Given the costs of the other intermediate products, the slope of the *NMR* curve depends on the slope of the marginal revenue curve for the final product[3] and thus on the slope of the final product's average revenue curve. At a given output, the greater is the slope of the firm's average revenue curve for its final product, the higher the price it would be prepared to pay for the intermediate product.

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[1] For example, on the first point, see E. A. G. Robinson, *Monopoly*, 1941, pp. 64 and 65; on the second, Stigler, *loc. cit.*, pp. 190 and 191.

[2] The maximum price (OP') at which the firm would be willing to buy in is given by the following formula, on the assumption that over the relevant range the *NMR* curve may be approximated by a straight line:

$$OP' = AC + \frac{1}{\tan a}\left[OC - AM \tan a - \sqrt{OC^2 - 20C.AM \tan a}\right]$$

where, as in Fig. 1, OC = the present output, and AC the average cost of Y, AM the difference between average cost and marginal cost, and a the slope of *NMR* measured from the vertical, at that output. The derivative of OP' with respect to OC is negative, with respect to AM is positive, and with respect to a is positive.

[3] See p. 35, n. 1.

If the elasticity of the average revenue curve is taken as an index of the degree of monopoly power enjoyed by a firm, the hypothesis is suggested that the extent to which decreasing average cost processes are vertically integrated increases with the degree of monopoly power.

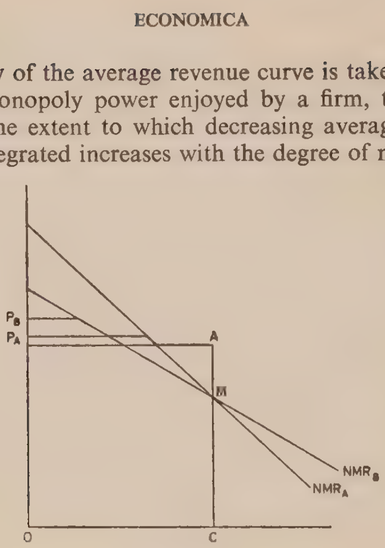

FIGURE 4

The argument can be put in diagrammatic form. In Fig. 4 two firms are represented. Both have the same cost curves and produce the same quantity of output *OQ*. Because the slopes of their average revenue curves differ, Firm A's *NMR* curve (NMR_A) is steeper than Firm B's (NMR_B). The maximum price which each firm would be prepared to pay to an outside supplier is determined in the manner described in section III; this is, say, OP_A for firm A, and OP_B for firm B. Thus at a price for the intermediate product in the range OP_A to OP_B, Firm A (the more " monopolistic ") would integrate and Firm B buy in.

A similar line of argument applied to the firm which is currently making at an output where both the average cost curve and the *NMR* curve are upward sloping leads to the opposite conclusion, namely that the more " monopolistic " the firm, the more likely it is to disintegrate the intermediate product. In Fig. 2, making the average revenue curve for the final product steeper at output OC has the effect of raising the *NMR* curve to the left of *M* and lowering it to the right of *M*. Thus the value of *OP′* falls.

VI

Adam Smith's famous generalisation that greater specialisation leads to increased wealth might cause one to expect that if the manufacture of a particular product became disintegrated its price would fall.

Let us first consider the case of the firm with a downward sloping average revenue curve for the final product. In terms of the model used in this paper this expectation is justified in the case of a firm which buys in an intermediate product which it has been making subject to increasing average costs. This can easily be seen from Fig. 3. The firm will find it profitable to buy in the process when the price of the intermediate product is below OP_1. At any price below OP_1 its output will be that at which the price of the intermediate product equals NMR. This will be greater than OQ, the output when all the intermediate product is made within the firm. Thus the price of the final product will fall.

However, if the firm has been making the intermediate product subject to decreasing average costs, disintegration may lead the firm to decrease its output and raise the price of the final product. From Fig. 1 it can be seen that only if the price of the intermediate product is less than an amount equal to MC will the firm expand its output beyond OC and lower the price of the final product. If the price of the intermediate product is between MC and OP' the firm will find it profitable to disintegrate the process, to reduce output below OC, and to raise the price of the final product.[1]

In the case of the final product made in conditions of perfect competition, disintegration may or may not lead to a fall in price. It will not, as in the case of imperfect competition just examined, lead to a rise in price, since a ceiling is set by the integrated firms.[2]

The London School of Economics.

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[1] I offer no opinion on what effect this will have on welfare.

[2] The implication that disintegration of a decreasing cost process would necessarily lead to a fall in price of the final product followed from Professor Stigler's argument that firms disintegrating the process would not pay more than their previous average costs of production.

Fred M. Taylor's Views on Socialism[1]

By Z. CLARK DICKINSON

In 1928 Fred Manville Taylor (1855–1932) served as president of the American Economic Association. His presidential address, " The Guidance of Production in a Socialist State ", is sometimes cited as a milestone in the history of socialist economics and occasionally as a partial demonstration of superiority of socialism over capitalism.[2]

In the present paper evidences will be presented as to the development of Taylor's information and attitudes on such matters over the last forty years of his teaching and scholarship. We shall see that his interest in leftist thought was much more of a theoretical model-building nature than of practical reforming. The purpose of his " Guidance " address is clarified herein, in part by means of a discarded introductory draft left in typescript by Taylor.

SOME OF HIS EARLIER STATEMENTS

Taylor's first collegiate teaching was at Albion College (Michigan), and chiefly in history, over somewhat more than a decade beginning in 1879. He obtained his Ph.D. in political philosophy in 1888 at the University of Michigan, whose faculty he joined in 1892, assuming a title in political economy and finance. Like such contemporaries as Richard T. Ely, John Bates Clark, and his colleague Henry Carter Adams, Taylor came from a religious family background and maintained high moral ideals throughout his life. Down to c. 1900 he devoted considerable efforts toward political improvements—in Albion municipal affairs and in national monetary reforms particularly. He assisted a few Albion students to work up research papers on the history and theory of illustrative reform movements, including the then-current silver agitation; three of these papers were printed in the college catalogue-yearbook. In 1896–98 he became an outstanding researcher and contributor to economic journals on currency elasticity.

When Taylor earned his Ph.D. at Michigan in 1888, he was acquainted with Henry Carter Adams, who had recently become full-time professor of political economy. Taylor's main interest then, however, was political philosophy; his dissertation entitled " The Right of the State to Be " was, in effect, a study in problems of sovereignty. Among other features Taylor propounded a rationale of evolutionary and revolutionary activities, perhaps somewhat reminiscent of Rousseau. He stated that his theory " justifies at once the honest rebel and the honest government which destroys him. (This explains [he continued] the entirely different sentiments which people of every age entertain toward political criminals . . . and common offenders . . .") The person contemplating revolutionary activity, said Taylor, should " weigh well the chances of failure, . . . realize that under the conditions of modern life, especially where the masses of men have some voice in government, it is almost certain that peaceful agitation will more surely and promptly secure the reign of justice. . . ."

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[1] The research of which this article is a partial report was assisted by a grant from the H. H. Rackham School of Graduate Studies, University of Michigan. The private papers of Professor Taylor, herein referred to, are in the Michigan Historical Collections of the same University; this unit's assistance is also gratefully acknowledged.

The author was one of his faculty colleagues over the last six years of Taylor's teaching.

[2] *American Economic Review*, Vol. XIX (1929), pp. 1–8. Taylor's essay was reprinted, along with papers by Oskar Lange, in the book edited by B. E. Lippincott, *On the Economic Theory of Socialism* (Minneapolis and New York, 1938). Cf. Joseph A. Schumpeter, *Capitalism, Socialism and Democracy*, p. 173 (1942 and later editions).

During his early years of teaching at Ann Arbor, in addition to courses in economic principles, money and banking, and industrial history, Taylor gave courses in theory and history of socialism and agrarian reforms. Among books he possessed at his retirement in 1929 were several by Richard T. Ely on labour and social movements and a dozen or so others with socialist slants, e.g. by Marx, Engels, Tugan-Baranowsky—and the Schæffle item mentioned below, p. 50.

No other indications have been found as to how he taught these courses on leftist movements; the next lines of evidence of his thoughts on such matters are passages in the nine editions of his *Principles of Economics* (1911–25). It should be borne in mind that revisions and rewritings such as are noted here were due mainly to pedagogical and printing considerations; not significantly to substantive changes in his attitudes.

PRESENT ORDER: SURVEY

The most obvious trails of his use of collectivist models are in chapters on " the present economic order "—general survey at the outset and critique at the end.

In the general survey he utilized four models which he called autonomous (Crusoe), communist (of Utopian nature), socialist, and " individual-exchange-co-operation "—the last being his favourite characterization of " the present order ". In his first edition (1911) the survey chapter contained the following language:

> . . . We have emphasized the thesis that there is regulation in the present order—that it is not given over to chaos; we must not neglect to disclaim any intention of characterizing the regulation actually effected as altogether just and expedient. . . . There surely are left not a few places where spontaneous regulation fails to attain good results; and it surely is possible that at some of these points authoritative regulation would do better. Finally, it is entirely possible that in the end organized society will come to look on the present system of regulation as so greatly inadequate that it will be constrained to adopt a system

> of complete authoritative regulation like communism, or one in which such regulation is only a little less complete, i.e. socialism. But even so, admitting the final unbearableness of the present order, we should still have to insist that this order is not chaotic, anarchic—that it is a regulated and a rationally regulated order, though one in which the process of regulation is automatic. . . .

In his second to fourth editions, inclusive, there was a marked expansion of exposition and analysis of elements of authoritative or legal control needed to make individual-exchange-co-operation work well; and of arguments that by no means all of the needful controls had yet been provided. The first edition's one-page concluding section on " Some Necessary Legal Conditions of the Present Order " was soon followed up (in following editions) by longer sections. One of these elaborations included a passage which, out of context, seems to indicate a leftist attitude:

> . . . There is no question whatever but that, if Michigan or the United States or Germany thinks best to adopt the most thoroughly socialistic or communistic system imaginable, it has a perfect right to do so. If society comes to believe that the system of automatic control is not, on the whole, working well, it will be the business of society to try something else."

There followed a note beginning:

> It is largely an error to suppose that the leading economists of the first half of the [nineteenth] century held opinions materially different from those just given. . . . It would be difficult to find a stronger and better statement [than in J. S. Mill's Book V, Chapter 1] of the principle that there is no proper criterion as to the legitimacy of any particular case of governmental interference except expediency.

All this was in line with the political philosophy of Taylor's Ph.D. dissertation written more than two decades earlier; and it is evident he took some pains to avoid posing as a stand-pat defender of all things as they were.[1]

These remarks were but little revised in Taylor's third and fourth editions (1914, 1916); but the book's form was considerably overhauled (in collaboration with Elmer C. Adams) for the fifth and following editions (1918 ff.). One change put the matter on " Authoritative Control in the Present Economic Order " into a separate Chapter III. In keeping with numerous other alterations in the text, the reference to Mill was omitted; as also passages such as are quoted above on the right of a *socialist* state to be. These changes appear adequately accounted for by Taylor's endeavour toward making his text more " teachable " to sophomores.

And in the new chapter he took occasion to add a few substantive touches on rationale of governmental action in the economic sphere.

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[1] Some of his departmental associates of the time, such as Henry C. Adams, Charles H. Cooley, Walton Hamilton, appear to have regarded Taylor as a backward looking conservative.

Some of them seem to show the influence of favourite ideas of Henry Carter Adams, e.g.:

> It has always been recognized that the present system needs to be supplemented by governmental action not only to increase its efficiency, but also to prevent undesirable consequences which the free working of the system would inevitably produce. One such consequence is the extreme *inequalities* in respect to the distribution of property income. In spite of the high efficiency of the system as a whole, many people feel that we cannot rest content until we have ameliorated the inequality resulting from it. . . .
>
> The type of interference in question is sometimes described as *determining the plane of competition*. . . .[1]

PRESENT ORDER: CRITIQUE

Among references to the history of economics which were deleted after the fourth edition were some concerning Karl Marx's value theory. In the earlier editions one such section was ended thus:

> Note 2. The student must not imagine that socialism as a project of reform stands or falls with the labor theory of value. The movement has not a few able advocates who frankly repudiate the Marxian economics. Socialism is essentially a scheme of social organization for economic ends. As such a scheme it deserves to be considered on its own merits, without respect to the entirely untenable doctrines with respect to the present order which its advocates have commonly held. Nevertheless, there can be no doubt that the general rejection of Marxian economics, which must surely come, will weaken the socialist movement. . . .[2]

Although various other parts of Taylor's first edition were, in effect, outlines which were written up in later editions, his concluding chapter, "A Critique of the Existing Economic Order", ran to 40 pages (total, 365). Through later editions numerous revisions and rearrangements were made, but the bulk grew only to 60 pages (of somewhat under 600). In all editions the general tenor of the " Critique " was favourable only to moderate and gradual reforms, somewhat reminiscent of Mill. Socialism, Taylor allowed, could probably mitigate if not abolish booms and depressions; but he thought the present order was making headway with mitigations.

The " Critique " chapters amplified somewhat remarks made earlier in the book as to needful further authoritative controls, notably as to incomes connected with property. Natural resources had often been improperly exploited to private advantage, he thought, and public conservation measures should be tightened. Numerous other acquisitions of income and property he regarded as indefensible, most of them connected with force, fraud, and monopoly. Normal profit and interest, however, he considered justified on the ground of marginal disutilities involved; and under competitive conditions, he said, " labour will tend to get all that portion of the joint output which is imputable to it as its product ".

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[1] Taylor's italics; he was addicted to them.

[2] The " general rejection " thus prophesied has doubtless occurred to a greater extent among influential socialist leaders than in popular propaganda. Such rejection as may have taken place has obviously occasioned less weakening of socialist tendencies than Taylor expected.

With respect to rent and speculation, he observed:

> . . . Wherever the element of chance, accident, plays a very great role, there is much to be said in favor of public ownership. While speculation performs a real economic function, . . . [it] is in many respects hurtful and demoralizing. A socialist state would need it much less than does the present order, in that the pooling of all industries would greatly diminish the risk element. Even going no further than to assume the control of land would do much to diminish risk and its attendant evils. . . . But . . . in view of the great superiority, in other respects, of private, to public, ownership, and in view of the fact that its worst evils can be gradually removed without overturning the system, we believe that the system of private ownership [of land and capital] should be maintained. At the same time, however, we believe that the regulation of private initiative should be carried much further than it has been, . . . and that at some points, how many and what only experience will show, *public ownership and initiative should be substituted for private.*[1]

" Guidance of Production " Address

" Like most teachers of economic theory ", began this presidential address in 1928, " I have found it quite worth while to spend some time studying any particular problem in hand from the standpoint of a socialist state. . . . Tonight, I am applying this method of procedure to a very fundamental problem of any co-operative economic order; that is, the problem embodied in this question: What is the proper method of determining just what commodities shall be produced from the economic resources at the disposal of a given community ? "

Taylor proceeded through a few more paragraphs to state the background and his procedure—all unrelievedly abstract in character. Under free private initiative, he said, there are in effect three major steps: (1) the consumer seeks money income; (2) he spends said income among " those persons who have voluntarily assumed the role of producers ", according to the consumer's choices; and (3) producers accept such " dictation, provided always that said citizen brings along with his demand entire readiness to pay for each commodity a price equal to [its] cost of production. . . . In the case of a socialist state, the proper method of determining what commodities shall be produced would be in outline substantially the same . . . "; the state would assure incomes subject to whatever conditions; and the citizen-consumer would therewith buy commodities and services produced by the state. Supposing that the public authorities would try to arrange distribution of incomes in the interest of the group as a whole, " it necessarily follows that the judgments reached by citizens with respect to the relative importances of different commodities would be virtually social judgments, and the resulting commodity prices would be prices which expressed the social importances of commodities ". (He was accustomed to argue that, in " the present order ", whatever economic activities are left in private hands, and the resulting prices, incomes, and consumption, are implicitly expressing social judgments as to relative importances.)

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[1] Taylor's italics. Quotations are from *Principles*, fourth edition (1916), pp. 451 ff. Substantially these views were maintained through all editions.

The primary factors of production, in any economic system, obviously include " land " and personal-service resources. For simplification Taylor did not in this paper mention abstinence or waiting and responsibility or risk-bearing as primary factors, though in all editions of his text he argued that these factors would, in some forms and degrees, exist in a socialist order. But in the short address he reflected a passage he especially admired in J. B. Say; as Taylor put it:

> . . . Every one of these numerous primary factors has its own particular degree or amount of effective importance in the vast complex of productive processes. . . . That effective importance . . . is derived from and determined by the importances of the innumerable commodities which emerge from that complex of productive processes. . . . It will be [here] assumed . . . that the authorities of our socialist state will have proved able to ascertain with a sufficient degree of accuracy these effective importances or values of all . . . primary factors, and that they will have embodied the results in arithmetic tables which I shall usually designate factor-valuation tables. . . .[1]

Socialist consumer prices, he said, should be kept in line with costs; divergence would amount to surreptitious tampering with the real-income-distribution on which the whole system rested.

The foregoing paragraphs have outlined what Taylor said was the " main task " he had set for himself in his address. He continued:

> In the course of that discussion it has probably become sufficiently evident why it would be necessary for the authorities of our socialist state to solve the so-called problem of imputation, that is, the problem of ascertaining the effective importance in the productive process of each primary factor. . . . It is not unlikely that more than one economist would question the possibility of solving that problem at all under the conditions necessarily prevailing in a socialist state. I seem called on, therefore, to give a few moments to show that, in fact, the socialist authorities would find themselves quite equal to this task. . . .

The remaining two pages of his essay in effect summarize his " automatic theory of imputation ", including adaptations to socialist conditions, as well as his other adaptations of doctrine he seems to have derived from von Wieser—who, perhaps, in turn was indebted to Walras. These reconstructions placed emphasis on trial-and-error pricings and their effects on quantities of factors and products supplied and consumed, rather than on the simultaneous equations which symbolize the theoretical equilibrium which given data would tend to produce.[1]

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[1] His appreciation that such " tables " would have to be frequently revised becomes clear in a moment; and we may add that such changes would, in effect, reflect state profits and losses. A major factor, of course, would be labour; and in his text and classes Taylor had argued that the socialist worker's income from labour would be likely to reflect his current position in factor valuation.

Supposing, as he did for his " Guidance " paper, that the stocks of primary factors are fixed or sluggish in changes, what cues would the authorities receive of incorrect valuations? Taylor's reply:

> If [these authorities] were actually using for any particular factor a valuation which was too high or too low, that fact would soon disclose itself in unmistakable ways. Thus, supposing that, in the case of a particular factor, the valuation given in the provisional factor tables was too high, that fact would inevitably lead the authorities to be unduly economical in the use of that factor; and this conduct, in turn, would make the amount of that factor which was available for the current productive period larger than the amount which was consumed . . ..

And *vice versa*, of course, for any too low provisional valuation. Changes of prices of factors (and, so far as needful, of products) would tend to proceed until the markets for all were cleared. Such adjustments and readjustments would occur only " if the economic authorities of a socialist state would recognize equality between cost of production on the one hand and the demand price of the buyer on the other as being the adequate and the only adequate proof that the commodity in question ought to be produced. . . ."

AIDS TO INTERPRETATION, IN TAYLOR PAPERS

It is evident that Taylor's address served as one of the replies to the argument, advanced by von Mises and others that, since in socialism there would be no private owners nor free market for land and capital, a socialist regime would be unable to accomplish a rational allocation of these factors and their services.[2] There are numerous evidences that Taylor did not regard his own demonstration as a statement of belief in the over-all feasibility and desirability of socialism; and from available materials a few are here exhibited to indicate how he happened to choose the subject of his presidential address. These bits, in his private papers, consist mainly of clippings pertaining to Soviet economic affairs and associated notes and short drafts of his own, including a discarded draft of introduction to his " Guidance " paper.

As to Soviet economic affairs in the early to middle 1920's, his major source was *The Economic Condition of Soviet Russia*, by Sergei N. Prokopovich, who seems to have held his professorship of economics at Moscow until he " was banished as late as June 1, 1922 ". A clipping which makes interesting reading now was the second of two articles by John M. Keynes—that which appeared in the *New Republic* of November 4, 1925. One shift of Bolshevik policy then evident corresponded to a long-favoured doctrine of Taylor's—that one thing needed by socialism would be money. Keynes also remarked on Leninism as " At the same time a persecuting and missionary religion and an experimental technique "; and he said, " After a long debate with Zinovieff, two Communist ironsides who attended him stepped forward . . . with the full faith of fanaticism in their eyes ". It was not yet apparent, of course, how this " persecuting and missionary religion " would develop.

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[1] Apparently Taylor gave no more than scanty attention to Walras on general equilibrium, and probably less to Edgeworth's criticisms of Walras. In the last edition of his text (1925), however, in connection with his own rewriting of imputation theory, Taylor did explicitly include entrepreneurial responsibility as a primary factor.

[2] Cf. F. A. von Hayek (ed.), *Collectivist Economic Planning* (1935), which contains, among other essays, Enrico Barone's " Ministry of Production in a Socialist State ". It is improbable that Taylor was familiar with this piece by Barone.

What interpretations did Taylor give to such reports? There are sheets which appear to be beginnings of lectures utilizing them. There are several pages of outlines bearing variants of the running heading " Economic Guidance in Production—How far present in Soviet Russia ? " One of these scrawls strikes the keynote of themes running through all: " A return to capitalism—the N.E.P. was temporarily anyhow a relinquishment of communism, even socialism ". This heading, and others, were followed by quotations from Prokopovich. Another note reads: " The Russian experience does not demonstrate the impossibility of a successful socialistic economy, but only such impossibility if economic calculation is rejected—if instead mere *gross utility* is accepted as the proper guide ". By " gross utility " he evidently meant estimates or guesses as to which classes of goods should outrank which others, in the use of productive resources—this view of desirable policy had long been a favourite whipping-boy in Taylor's teaching.

In another of the trial runs in his notes some further significant points appear:

> RULES FOR GUIDANCE OF SOCIALIST AUTHORITIES BASED ON ECONOMIC PRINCIPLES.
>
> 1. Any sharing in the social income shall be based on contributing to the production of such income. (Of course exceptions) . . .
>
> 3. In the tables of the accounting department which show the valuation of factors:
>
> (*a*) The valuation of each *primary* factor shall be in accord with the effective importance of its participation in the productive process as a totality.
>
> (*b*) The valuation of each *produced* factor shall be equal to its cost of production.
>
> 4. Those products which can be sold at a price covering their costs of production (including or not profit) and *only* such products shall be produced. . . .
>
> 7. The prices paid by the state for productive services shall be such as are necessary to secure the best distribution of human capacities among the different types of services. . . .

THE UNUSED INTRODUCTION

From the foregoing fragments we may proceed to major portions of a more relevant typescript which evidently was a tentative draft:

> THE GUIDANCE OF PRODUCTION IN A SOCIALIST STATE
>
> . . . In my graduate student days, someone recommended to me Schaeffle's *Quintessence of Socialism* as a very informing and suggestive presentation of the socialist program. I . . . read it with much profit. Among many good points, one in particular made the greatest immediate impression. That point was that the economic authorities of a socialist state would not find it practicable to fix the prices of their products according to the labor cost of producing these products—a plan of procedure which was then commonly attributed to the supporters of socialism.
>
> Schaeffle's argument in support of his contention was quite brief, and, re-read [in 1928], it seems scarcely adequate to establish that contention. But . . . it had suggested . . . a more general proposition, or rather a composite of several propositions which were disengaged from one another only little by little. That composite . . . might perhaps be resolved into these three particular propositions: (1) Under any economic order, value—price—must be the very heart of the mechanism through which economic affairs are regulated if that order is to realize any measure of success. (2) The fundamental principles which determine value—price—would be substantially the same under any economic order. (3) One of the best ways of studying these fundamental principles is to imagine ourselves acting as the economic authorities of a socialist state, and to be engaged in working out on the basis of common sense, a body of rules which might reasonably be expected to insure our making the best use of the economic resources at our disposal.[1]

The last of these themes was reiterated in the discarded draft, and very similar language appeared in the final paper. Among the discards was the statement that, in testing procedures and results of well managed capitalism by comparisons with imagined well managed socialism, " I think of myself as using a method which has been at least implicit in much of the best work from Adam Smith down . . . [and] if some one smiles broadly at such an appeal to the practices of dreamland in dealing with very real problems, I console myself with the thought that I am on the whole in excellent company ".

Another point noticeable in comparing the tentative with the final draft is greater prominence in the unused introduction of Wieserian language—" That problem I have designated in the title of this paper as the problem of imputation. . . ." Perhaps here was a forgetful carry-over of an earlier title-intention, since the unused draft actually bore the same heading as the final one.

Taylor's copy, thus referred to, of Schæffle's *Quintessence* bears many marks of Taylor's pencil, probably scribbled there in the middle 1890's when he was teaching history and theory of socialism. His marginal comments are mostly adverse; e.g. against the Austrian's remark, " Socialism . . . can no longer allow trading and markets, and it would be necessary even for coinage eventually to cease to exist and for labor-money to take its place ", Taylor marked " Rubbish ! " (Schæffle, more sociologist than economist, was at one time Finance Minister.)

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[1] " I say ' best ways ', because of the fact that the system of free private initiative —everyone free to start things—has to depend on automatic working of competition to secure rationality, whereas socialism would go directly to its goal, etc."

As shown above, Taylor's long-continued habit of using comparative economic systems in teaching economic theory made him a very interested reader about Soviet economic affairs, reports on whose first decade undoubtedly played a part in his choice of topic for this presidential address. Obviously he knew that the Soviet regime was not behaving and might never behave in accord with principles set forth in his " Guidance " address. And it must remain conjectural whether he had Mises or any other specific person in mind when he wrote, " It is not unlikely that more than one economist would question the possibility of solving [the imputation] problem at all under the conditions necessarily prevailing in a socialist state ". Already by 1911 he thought he had detected and corrected a serious inadequacy in Wieser's handling of imputation—and incidentally, to the end of his days he considered that Wieser made too many concessions to socialism. His " Guidance " paper represents his final wrestling with imputation theory, putting it at greater length into a comparative-economic orders setting.

The context, especially of the unused introductory remarks, shows that he was not passing judgment as to whether any society claiming or meriting the designation socialist would actually consider it " natural and reasonable " to behave in the manner prescribed by him. Much in the manner of all editions of his text, socialist problems and possibilities were outlined and analogies with " capitalism " were explored —notably use of money, trial-and-error pricing to clear markets in relation to incomes and disposals of consumers. Implicitly the socialist state of his address would operate through state-owned enterprises as well as state-owned land, buildings, and other capital forms; and the enterprises apparently would have considerable autonomy as to dealings with each other.[1] His private notes on " The Russian Experiment " indicate that he gave attention almost exclusively to developments which tended to confirm his previous opinions as to similarities between capitalist and socialist fundamentals. He gave little or no consideration to the anomalous behaviour of the Soviet authorities in allocations of land, buildings, and other capitals, nor to the very limited role of " consumer sovereignty ".[2]

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[1] No. 6 of his pencilled " Rules for Guidance of Socialist Authorities ", referred to above, was: " All authoritative valuations and all market prices shall be subject to the rule of single price, i.e. each unit of any kind of factor, or service, or product shall have the same valuation or price as any other unit."

[2] The last (No. 8) of the " Rules " fragment is simply, " The special case of capital " —evidently he did not yet have a prescription ready for socialist capital formation, and perhaps never found one to suit him.

We may well suppose that Taylor, like Schumpeter, expected that Britain would be the major proving ground for attempted rational and democratic socialism. Taylor did, in fact, make numerous notes on Norman Angell's anti-Trotsky book, *Must Britain Travel the Moscow Road ?*—mostly, however, as to similarities of capitalist and socialist fundamentals; not much on Angell's pro-Labourite views. Over all, the practical reforms favoured by Taylor were quite on the gradualist and free-enterprising side. His " Guidance " paper appears to have been intended, in part, to illuminate one basic problem of any society; and perhaps, in part, to warn that impossibility-of-rational-allocation-of-resources is not a sound indictment of any and every sort of socialism.

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A Taxonomic Note on the Multiplier and Income Velocity

By R. F. G. ALFORD

There has recently been discussion of some aspects of the relationship between the multiplier and velocity (by which we shall always here mean income velocity).[1] The purpose of this note is to set out the relationship between the two as it stands in current economic theory.

The method used here is to set up a comparative equilibrium Keynesian model and to derive from it equations for multipliers and velocities below and at full employment. This Keynesian model at full employment is then modified into a quantity theory model, and from some of the equations derived above we then derive a multiplier and velocities for this quantity theory model. The relationship between the multiplier and velocity is then briefly summed up in the light of these results.

A KEYNESIAN MODEL

The Keynesian system is founded upon two identities. The first of these is:

$$Y \equiv C + I \quad \ldots\ldots(1)$$

which defines the flow of income, Y, received in a period as the sum of the flows of consumption expenditure, C, and investment expenditure, I, in that period. The second identity is:

$$M \equiv M_1 + M_2 \quad \ldots\ldots(2)$$

which defines the stock of money, M, in an economy at any time as being composed of two parts, that held for the transactions and precautionary motives, M_1, and that held for the speculative motive, M_2. Implicit in these identities are the marginal forms $\triangle Y \equiv \triangle C + \triangle I$ (1a) and $\triangle M \equiv \triangle M_1 + \triangle M_2$ (2a), which are given separate labels for convenience.

The behaviour equations of the comparative equilibrium Keynesian model we use here are:

$$C_R = \bar{C}_R + C'Y_R \quad \ldots\ldots(3')$$

$$I_R = \bar{I}_R - I'_R r \quad \ldots\ldots(4')$$

$$M_1 = L_1'Y \quad \ldots\ldots(5)$$

$$M_2 = \bar{L}_2 - L_2'r \quad \ldots\ldots(6)$$

$$M = \bar{M} \quad \ldots\ldots(7)$$

[1] Vera Lutz, " Multiplier and Velocity Analysis: A Marriage ", *Economica*, February, 1955; G. C. Archibald, " Multiplier and Velocity Analysis: An Annulment ", *Economica*, August, 1956; E. J. Mishan, " Notes on the Place and Significance of the Money Multiplier ", *Economica*, August, 1957.

The first two of these, the consumption function and the investment function, are both explicitly in real terms, shown by the subscript $_R$. The terms on the right-hand sides of these five behaviour equations are given terms (shown with a bar) or varying terms made up of a constant coefficient (C', $-I'_R$, etc.), and a term for real income, Y_R, money income, Y, or the rate of interest, r. The first four functions are thus linear but in other respects they all conform to the standard Keynesian configuration,[1] and the quantity of money is here held constant.

In this Keynesian model we shall make the simplest possible assumption about the behaviour of the price level—that it is constant below full employment, and that at full employment, where real income is constant, the price level can rise but cannot fall.

THE MULTIPLIER BELOW FULL EMPLOYMENT

Considering first a situation of less than full employment, to deal with the determination of money income behaviour equations (3′) and (4′) must be converted from real into money terms. This is done at a base price level and the subscript $_R$ can then be dropped from the two equations which are then in money expenditure terms. To allow for any rise in the price level (which must have occurred at full employment) from this base we multiply the appropriate terms by p, the ratio of the current price level to the base price level. In place of equations (3′) and (4′) in real terms we then have at the current price level the money expenditure equations:

$$C=\bar{C}p+C'Y \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3)$$

$$I=\bar{I}p-I'pr \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (4)^2$$

Implicit in behaviour equations (3) to (7) are the marginal forms $\triangle C=C'\triangle Y$..(3a), $\triangle I=-I'p\triangle r$..(4a), $\triangle M_1=L_1'\triangle Y$....(5a), $\triangle M_2=-L_2'\triangle r$.. (6a) and $\triangle M=$ zero .. (7a).

The multiplier is defined here as:

$$mk\equiv\frac{\triangle Y}{\triangle \bar{I}p} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (8)$$

where $\triangle Y$ is the change in the equilibrium level of money income caused by $\overline{\triangle I}p$ a change in the given term of the investment expenditure function at current prices, equation (4), reflecting a change in the given term of the real investment function, equation (4′). Here we shall distinguish the multiplier below full employment, *mk*1, and the multiplier at full employment, *mk*2.[1]

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[1] For simplicity we shall speak as if the rate of interest is above its minimum level at which $-L_2'$ is infinite; this latter case can be included without any difficulty.

[2] With $p=1$, this investment function is of the sort normally used below full employment. At full employment we shall assume that, with given expectations, any price rise affects equally the costs and estimated returns of any investment project, so that the rise in prices can be expected to have no direct effect on real investment; we therefore correct the investment expenditure function to take account of any price rise. This seems to be more consistent than the common practice of taking the investment function as given in money terms at full employment, so that a price rise does directly affect real investment.

To derive *mk*1, $\overline{\Delta I}p$ is introduced into behaviour equation (4a):

$$\Delta I = \overline{\Delta I}p - I'p\,\Delta r \quad \text{.........................}(4b)$$

Substituting from equations (3a) and (4b) into identity (1a) and from equations (5a), (6a) and (7a) into identity (2a) we have two equilibrium equations with two unknowns, ΔY and Δr:

$$\Delta Y = C'\Delta Y + \overline{\Delta I}p - I'p\,\Delta r \quad \text{......................}(9)$$

$$0 = L_1'\Delta Y - L_2'\Delta r \quad \text{..............................}(10)$$

From (10):

$$\Delta r = -\frac{L_1'\Delta Y}{-L_2'} \quad \text{................................}(11)$$

Substituting from equation (11) into equation (9):

$$\Delta Y = C'\Delta Y + \overline{\Delta I}p - I'p\left(\frac{L_1'\Delta Y}{L_2'}\right)$$

$$1 = C' + \frac{\overline{\Delta I}p}{\Delta Y} - \frac{I'pL_1'}{L_2'}$$

$$\frac{\Delta Y}{\overline{\Delta I}p} = \frac{1}{1 - C' + \dfrac{I'pL_1'}{L_2'}} \quad \text{......................}(12)^2$$

THE MULTIPLIER AT FULL EMPLOYMENT

Turning to a situation of full employment, real income cannot rise but prices can, so that any increase in money income occurs in the price dimension only. Since prices cannot fall at full employment, the multiplier at full employment relates only to a rise in the investment function.

If at full employment real income is constant at $\bar{Y}_R$, then from equation (3′) the equilibrium ratio between real consumption and real income is:

$$\frac{C_R}{\bar{Y}_R} = \frac{\bar{C}_R}{\bar{Y}_R} + C' \quad \text{................................}(3'')$$

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[1] Throughout we shall use the symbol *m* for marginal relationships and *a* for average ones, while relationships at less than full employment will be denoted by 1, and those at full employment by 2. Thus the symbol for the multiplier is expanded from the usual *k* to show clearly that it is a marginal relationship and that it relates to a specific employment situation. The inclusion of *m* or *a* may be tautological, but it seems worth while putting up with this for the added clarity.

[2] The version of *mk*1 most commonly used can be interpreted as a special case of equation (12) in which $p=1$ and disappears from the right-hand side, and the fractional term in the denominator of the right-hand side disappears, leaving $\frac{\Delta Y}{\overline{\Delta I}} = \frac{1}{1-C'}$. The assumption behind the disappearance of this fractional term could be that $-I'$ is equal to zero, or that $-L_2'$ is infinite (so that Δr is equal to zero), or that ΔM is equal not to zero but to $L_1'\Delta Y$ (which again would make Δr equal to zero). It is seldom clear which of these is being assumed.

and we shall call this constant ratio C″. To maintain real consumption at its equilibrium level as money income rises, consumption expenditure must be kept a constant proportion C'' of money income. The consumption expenditure function at full employment is therefore:

$$C=C''Y \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots \text{(3b)}$$

of which the marginal form is $\Delta C=C''\Delta Y$ ....(3c).

Equation (4) gives the investment expenditure function at full employment, but now instead of the marginal form (4a) which allows only for changes in expenditure due to changes in the rate of interest we require one which allows also for a change in p. The marginal form of the investment expenditure function at full employment is therefore:

$$\Delta I=(p+\Delta p)(-I'\Delta r)+\Delta p(\bar{I}-I'r) \quad \dots\dots\dots\dots \text{(4c)}$$

where on the right hand side, the first term is the right-hand side of equation (4a), which is at current prices, corrected for any further change in p, and the second term is the right-hand side of equation (4), the initial equilibrium level of investment, but taken at base prices and corrected for any change in p from the current level.

To derive *mk*2, $\overline{\Delta Ip}$ is introduced into equation (4c):

$$\Delta I=(p+\Delta p)(\overline{\Delta I}-I'\Delta r)+\Delta p(\bar{I}-I'r) \quad \dots\dots\dots\dots \text{(4d)}$$

Equation (11) is unaffected by the transition to full employment, and substituting from equation (11) into (4d) and from this and (3c) into identity (1a) we have:

$$\Delta Y=C''\Delta Y+(p+\Delta p)\left(\overline{\Delta I}-\frac{-I'L_1'\Delta Y}{-L_2'}\right)+\Delta p(\bar{I}-I'r)$$

Taking full employment real income $\overline{Y}_R$ at base prices we have $\overline{Y}$, and in place of Δp in the last term we can then have $\frac{\Delta Y}{\overline{Y}}$, so that:

$$1-C''=(p+\Delta p)\left(\frac{\overline{\Delta I}}{\Delta Y}-\frac{I'L_1'}{L_2'}\right)+\frac{\bar{I}-I'r}{\overline{Y}}$$

$1-C''$ is the equilibrium ratio of saving to income at full employment and $\frac{\bar{I}-I'r}{\overline{Y}}$ is the equilibrium ratio of investment to income, at base prices, at full employment. These must be equal and they can therefore be deducted from their respective sides:

$$0=(p+\Delta p)\left(\frac{\overline{\Delta I}}{\Delta Y}-\frac{I'L_1'}{L_2'}\right)$$

$$\frac{\Delta Y}{\overline{\Delta I}}=\frac{1}{\frac{I'L_1'}{L_2'}}$$

$$\frac{\Delta Y}{\overline{\Delta Ip}}=\frac{1}{\frac{I'pL_1'}{L_2'}} \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots \text{(13)}$$

In Keynesian short-period analysis attention is concentrated upon shifts in the investment function and the multiplier as the explanation of changes in income, and the multiplier means either $mk1$ or $mk2$, depending upon the employment situation. It will be seen that there is a term for the marginal propensity to save, $1 - C'$, in the denominator of the right hand side of equation (12) which is absent in equation (13). Below full employment a rise in the investment function raises real and money income and saving, while the induced rise in the rate of interest moderates the impact rise in real and money investment; the equality of (*ex ante*) real saving and investment is characteristic of equilibrium and these two forces tending to bring them into equality both appear in the denominator of the right-hand side of equation (12). But at full employment in this Keynesian model money saving rises in proportion with money income so that real saving is unchanged. Then it is only the effect on investment of the induced rise in the rate of interest which brings real saving and investment into equality, and the saving term disappears from the multiplier, as shown on the right-hand side of equation (13).

$mk1$ and $mk2$ are both money income multipliers, the change in money income arising from a change in the investment expenditure function, which in turn reflects a change in the real investment function. But while $mk1$ is equally a real income multiplier, $mk2$ is not, for with real income constant at full employment no change in the real investment function can affect it, and the real income multiplier must be zero. It will be noted that as a real income multiplier, $mk1$ contains p; this reflects the fact that a given change in real income will induce a change in the rate of interest which will depend upon the price level at which the change in real income occurs, M being constant.

VELOCITIES IN A KEYNESIAN MODEL

Equilibrium equations for velocities can be derived for this Keynesian model in the same way that equilibrium equations for multipliers were derived above. Here we shall define marginal and average velocities:

$$mv \equiv \frac{\Delta Y}{\Delta M} \qquad \text{(14)}$$

$$av \equiv \frac{Y}{M} \qquad \text{(15)}$$

and we shall distinguish marginal velocity below full employment, $mv1$, and at full employment, $mv2$, and average velocity below full employment, $av1$, and at full employment, $av2$.

Starting with $mv1$, $\overline{\Delta M}$ is a change in the quantity of money, and this is introduced into behaviour equation (7a) which becomes:

$$\Delta M = \overline{\Delta M} \qquad \text{(7b)}$$

Substituting from behaviour equations (3a), (4a), (5a), (6a) and (7b) into identities (1a) and (2a) we have the equilbrium equations:

$$\triangle Y = C'\triangle Y - I'p\triangle r \quad \text{.........................(16)}$$

$$\overline{\triangle M} = L_1'\triangle Y - L_2'\triangle r \quad \text{.......................(17)}$$

From equation (17):

$$\triangle r = \frac{\triangle M - L_1'\triangle Y}{-L_2'} \quad \text{...........................(18)}$$

and substituting from equation (18) into equation (16):

$$\triangle Y = C'\triangle Y - I'p\,\frac{\overline{\triangle M} - L_1'\triangle Y}{-L_2'}$$

$$1 = C' + \frac{-I'p\overline{\triangle M}}{-L_2'\triangle Y} - \frac{-I'pL_1'}{-L_2'}$$

$$\frac{-L_2'\triangle Y}{-I'p\overline{\triangle M}} = \frac{1}{1 - C' + \dfrac{-I'pL_1'}{-L_2'}}$$

$$\frac{\triangle Y}{\overline{\triangle M}} = \frac{\dfrac{I'p}{L_2'}}{1 - C' + \dfrac{I'pL_1'}{L_2'}} \quad \text{.........................(19)}$$

To derive *mv*2 we substitute from equations (3c), (4c) and (18) into identity (1a):

$$\triangle Y = C''\triangle Y + (p + \triangle p)\left(-I'\,\frac{\overline{\triangle M} - L_1'\triangle Y}{-L_2'}\right) + \triangle p(\bar{I} - I'r)$$

Substituting $\frac{\triangle Y}{\bar{Y}}$ for $\triangle p$ in the last term:

$$1 - C'' = (p + \triangle p)\left(\frac{-I'\overline{\triangle M}}{-L_2'\triangle Y} - \frac{-I'L_1'}{-L_2'}\right) + \frac{\bar{I} - I'r}{\bar{Y}}$$

Since $1 - C''$ must equal $\frac{\bar{I} - I'r}{\bar{Y}}$:

$$\frac{-L_2'\triangle Y}{-I'\overline{\triangle M}} = \frac{1}{\dfrac{-I'L_1'}{-L_2'}}$$

$$\frac{\triangle Y}{\overline{\triangle M}} = \frac{1}{L_1'} \quad \text{..................................(20)}$$

Average velocity below full employment, *av*1, is derived by substituting from equations (3), (4), (5), (6) and (7) into identities (1) and (2):

$$Y = \bar{C}p + C'Y + \bar{I}p - I'pr \quad \text{........................(21)}$$

$$\bar{M} = L_1'Y + \bar{L}_2 - L_2'r \quad \text{...........................(22)}$$

From equation (22):

$$r=\frac{\bar{M}-\bar{L}_2-L_1'Y}{-L_2'} \quad \text{.............................(23)}$$

and substituting from equation (23) into equation (21):

$$Y=\bar{C}p+C'Y+\bar{I}p-I'p\,\frac{\bar{M}-\bar{L}_2-L_1'Y}{-L_2'}$$

from which:

$$\frac{Y}{\bar{M}}=\frac{\dfrac{\bar{I}p+\bar{C}p}{\bar{M}}+\dfrac{I'p}{L_2'}\left(1-\dfrac{\bar{L}_2}{\bar{M}}\right)}{1-C'+\dfrac{I'pL_1'}{L_2'}} \quad \text{..................(24)}$$

Average velocity at full employment, *av*2, is derived by substituting from equations (3b), (4) and (23) into identity (1):

$$Y=C''Y+\bar{I}p-I'p\,\frac{\bar{M}-\bar{L}_2-L_1'Y}{-L_2'}$$

from which:

$$Y=\frac{\bar{I}p-(1-C'')Y+\dfrac{I'p}{L_2'}(\bar{M}-\bar{L}_2)}{\dfrac{I'pL_1'}{L_2'}} \quad \text{................(25)}$$

From equation (3″) we have $C''=\dfrac{\bar{C}_R}{\bar{Y}_R}+C'$ and the terms in the fraction on the right-hand side can be taken at current prices by dropping the subscript $_R$ (so that they are at base prices) and multiplying by p; we then have $C''=\dfrac{\bar{C}p}{\bar{Y}p}+C'$ and $\bar{Y}p$ is equal to Y. In place of $-(1-C'')Y$ in equation (25) we can then have $-\left(1-\dfrac{\bar{C}p}{\bar{Y}p}-C'\right)\bar{Y}p$ or $\bar{C}p-(1-C')\bar{Y}p$ so that:

$$Y=\frac{\bar{I}p+\bar{C}p-(1-C')\bar{Y}p+\dfrac{I'p}{L_2'}(\bar{M}-\bar{L}_2)}{\dfrac{I'pL_1'}{L_2'}}$$

Then all p's cancel out on the right-hand side, and dividing by $\bar{M}$:

$$\frac{Y}{\bar{M}}=\frac{\dfrac{\bar{I}+\bar{C}-(1-C')\bar{Y}}{\bar{M}}+\dfrac{I'}{L_2'}\left(1-\dfrac{\bar{L}_2}{\bar{M}}\right)}{\dfrac{I'L_1'}{L_2'}} \quad \text{............(26)}$$

The two multipliers and four velocities derived for our Keynesian model are brought together in Table I. With constant coefficients and a given value of p, we have constant values of $mk1$, $mv1$, $mk2$ and $mv2$. It will be seen that as $\overline{M}$ rises the value of $av1$ must be equal to, or approaching (from above or below) the value of $mv1$, and that the value of $av2$ must be equal to the value of $mv2$ or approaching it from below (depending upon whether $M_2=0$ or $M_2>0$ respectively). Marginal velocities are seldom used in the theory of income determination and velocity can be taken to mean average velocity. It will be

TABLE I

	Below full employment	At full employment
Multipliers	$mk1=\dfrac{1}{1-C'+\dfrac{I'pL_1'}{L_2'}}$ ......(12)	$mk2=\dfrac{1}{\dfrac{I'pL_1'}{L_2'}}$ ....(13)
Velocities	$mv1=\dfrac{\dfrac{I'p}{L_2'}}{1-C'+\dfrac{I'pL_1'}{L_2'}}$ ......(19)	$mv2=\dfrac{1}{L_1'}$ ....(20)
	$av1=\dfrac{\dfrac{\bar{I}p+\bar{C}p}{\overline{M}}+\dfrac{I'p}{L_2'}\left(1-\dfrac{\overline{L_2}}{\overline{M}}\right)}{1-C'+\dfrac{I'pL_1'}{L_2'}}$ ..(24)	$av2=\dfrac{\dfrac{\bar{I}+\bar{C}-(1-C')\bar{Y}}{\overline{M}}+\dfrac{I'}{L_2'}\left(1-\dfrac{\overline{L_2}}{\overline{M}}\right)}{\dfrac{I'L_1'}{L_2'}}$.(26)

TABLE Ia

At full employment
$mk2'=0$
$mv2'=\dfrac{1}{L_1'}$
$av2'=\dfrac{1}{L_1'}$

TABLE II

Active factor	Keynesian model (at full employment, expansion only)	Quantity theory model (expansion or contraction)
$\bar{I}$	$mk2=\text{constant}>0$ $av2$ varies	$mk2'=0$ $av2'=\text{constant}>0$
$\overline{M}$	$mv2=\text{constant}>0$ $av2$ probably varies	$mv2'=av2'=\text{constant}>0$

seen that the inclusion of "average multipliers" *ak*1 and *ak*2 would give symmetry to Table I. Such average multipliers can be defined and derived for our Keynesian model, but they appear to have no significance.

THE KEYNESIAN MODEL AND A QUANTITY THEORY MODEL

There are a number of differences between the comparative equilibrium Keynesian model used above and a comparative equilibrium model of the quantity theory; one is that the Keynesian model deals with both full employment and less than full employment equilibrium, while the usual quantity theory model relates only to full employment equilibrium. But if we restrict the comparison to one between the Keynesian model at full employment and a quantity theory model, then so far as the broad outline of the theory of income determination is concerned, there is only one basic difference between the two. In the Keynesian model there is uncertainty about the future level of the rate of interest which shows itself in the existence of the L_2 function and M_2 balances. In a quantity theory model, on the other hand, all money is always in M_1 (using Keynesian terminology). But the two theories have the same treatment of equilibrium holdings of M_1—they are proportional to money income. Taking the Keynesian model at full employment, therefore, we can modify it into a quantity theory model by removing the L_2 function (and all the typically Keynesian mechanism). Then setting $\overline{L}_2$ and $-L_2'$ equal to zero in the equations for *mk*2, *mv*2 and *av*2 we derive equations for a multiplier and a marginal and an average velocity in a quantity theory system, which we shall denote by *mk*2′, *mv*2′ and *av*2′. These are shown in Table Ia, and since prices in a quantity theory model will be flexible in both directions, these equations hold for both expansions and contractions in money income.

THE MULTIPLIER AND VELOCITY

The multiplier and velocity are significant for the roles they play in theories of income determination. The chief differences between them are that the multiplier is a marginal relationship between changes in the investment function (flow) and changes in income (flow) while velocity is an average relationship between the quantity of money (stock) and income (flow). They play their significant roles in models which represent different situations in which these two different factors, the investment function and the quantity of money, have the active role in determining income. But the relationship between the multiplier and velocity can be clarified by comparing the two in the Keynesian model at full employment and in the quantity theory model. This comparison is summarised in Table II.

In the Keynesian model attention is concentrated upon shifts in the investment function as the active factor which affects money income at full employment through the multiplier *mk*2 (which is constant in the model used here). Such a change in income will be associated with a redistribution of the given quantity of money between M_1 (where it has a constant income velocity of $\frac{1}{L_1'}$) and M_2 (where it has a constant income velocity of zero), and a change in velocity *av*2.

However, there is nothing in the Keynesian model to rule out a change in the quantity of money as an active factor, and at full employment this could be explained as affecting money income through marginal velocity *mv*2 (which is constant in this model).

It will be seen from Table I that:

$$mv2 \equiv \frac{\triangle Y}{\overline{\triangle M}} = \frac{1}{L_1'} = \frac{\dfrac{I'p}{L_2'}}{\dfrac{I'pL_1'}{L_2'}}$$

$$\text{and} \quad \frac{\triangle Y}{\overline{\triangle M}\,\dfrac{I'p}{L_2'}} = \frac{1}{\dfrac{I'pL_1'}{L_2'}} = mk2$$

A simple interpretation is that $\overline{\triangle M}\,\frac{I'p}{L_2'}$ is the induced impact change in investment expenditure due to a movement along a given investment expenditure function, following a change in the rate of interest caused by $\overline{\triangle M}$ occurring in M_2. This induced change in investment expenditure affects money income through the multiplier *mk*2. In the Keynesian model, with $M_2 >$ zero, *av*2 will vary as the quantity of money changes.

Constancy of the relationship between the quantity of money and money income, velocity *av*2′ (which is therefore equal to *mv*2′) is the core of the quantity theory model; the quantity of money is then the sole active factor determining money income. In this model changes in the investment function cannot affect money income and *mk*2′ is equal to zero.[1]

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[1] Using the same method as above, a table can be set out which is parallel to Tables I and IA, but which shows the average and marginal relationships between the constant term in the investment function and the quantity of money on the one hand, and the rate of interest (instead of income) on the other. This table shows that for both the Keynesian model at full employment and the quantity theory model, $\frac{\triangle r}{\triangle I} = \frac{1}{I'}$ and $\frac{\triangle r}{\overline{\triangle M}} =$ zero.

Mrs. Robinson's Dynamics

By Kelvin Lancaster

Any work by Mrs. Robinson must command respect, and when she writes a book[1] which seems intended to be as important a contribution to the theory of growth and economic dynamics as her *Imperfect Competition* was to the theory of price, then such a book can be judged only by the highest standards. Merely to choose pieces of such a work and say, this piece is good, this piece is bad, would be an insult to its author's intentions. The book must be judged as a whole, in terms of its fabric, its method and its general sweep: in terms of these, it does not succeed. Why not?

The chief fault of the book might seem to be failure of communication, for there can be no doubt that Mrs. Robinson's own view of what she is doing (" The whole argument is set out, as far as possible, as an analytical construction, with a minimum of controversy "[2]) does not accord with the impression gained by many of her readers, to whom the way she has chosen to write gives an air of controversy even when she is being uncontroversial. Professor Lerner has likened the work to " a pearl whose most conspicuous product is irritation ".[3]

Even, however, ignoring Mrs. Robinson's incredibly numerous *obiter dicta*, aphorisms, paradoxes and parables, and so concentrating on the underlying analysis, one still finds difficulties in following the central thread. Dr. Klein, reviewing the book, thought that it contained " many brilliant flashes of insight, but, unfortunately, does not seem to lead anywhere in particular "[4]; but this might have been said of so many books that one cannot condemn Mrs. Robinson solely on the ground of not arriving—after all, it is the journey that counts most in work of this kind. Nor can one regard the analysis in the book as being, in itself, inherently difficult, although some writers have found it so. Again, this may appear as a mere problem in communication, in didactic art. Indeed, commenting on Mr. Worswick's recent discussion[5] of her basic model, Mrs. Robinson would seem to admit failure of her exposition : " I am very sorry that my book should be so difficult. What I meant is quite simple, but I evidently failed to make it clear ".[1]

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[1] Joan Robinson, *The Accumulation of Capital*, Macmillan & Co. Ltd., 1956, xvi, 440 pp., 28s.

[2] *Ibid.*, p. x.

[3] Review of " The Accumulation of Capital ", *American Economic Review*, vol. XLVII (1957), p. 693. Lerner's figure of speech was derived, by inversion, from a comment by Solow (*Review of Economic Studies*, vol. XXIII [1955–56], p. 101) on an article by Mrs. Robinson (" The Production Function and the Theory of Capital ", *Review of Economic Studies*, vol. XXI [1953–54] p. 81) similar in style and content to her book.

[4] L. R. Klein, *Econometrica*, vol. XXVI (1958), p. 622.

[5] G. D. N. Worswick, " Mrs. Robinson on Simple Accumulation ", *Oxford Economic Papers*, vol. II (1959), p. 125.

The failure of *The Accumulation of Capital*, compared with the success of *Imperfect Competition*, has not been due to any decline in Mrs. Robinson's powers of exposition between the two works, but quite simply to the fact that her technique of analysis, which suited the comparative static approach of the earlier book, is not at all well suited to the handling of the dynamic problems of capital. It is not Mrs. Robinson who has failed, but the " words and pictures " tradition which is inadequate to the task of dynamic analysis in economics.

Perhaps the most compelling demonstration of the inadequacy of the verbal approach is merely to think a little deeply about one of the arguments put forward in its favour. Dr. Barna, reviewing Mrs. Robinson's book, wrote: " As against the approach of the mathematicians, Mrs. Robinson's technique can bring theory nearer to reality ".[2] Surely, whatever else one may say of Mrs. Robinson's book, it has little to do with reality, but is concerned with the discussion of a highly simplified, abstracted economic model in which relationships are asserted or assumed, but are never shown to exist in any real economy, and in which the only figures are those which Mrs. Robinson invents for her numerical examples. That Dr. Barna, or Mrs. Robinson herself, should fall into the simple semantic error of supposing that, because the words she uses are also used in other contexts to describe real phenomena, Mrs. Robinson's treatment is realistic, is clear demonstration of the dangers of the method. Mathematical methods, properly used, should never lead into this kind of error; even here, however, the " arts " tradition of jumping straight from a simplified abstract analysis to a Grand Conclusion about The Real World proves too strong for the more austere scientific caution.

. . . .

Consider Mrs. Robinson's fundamental model, that on which the whole of her book, in so far as it can be considered a coherent argument, must rest. This is her model of long-run accumulation with a constant technique, set out in chapter 8. The assumptions are set out clearly enough: there is an economy with two sectors, one producing capital goods, the other consumer goods, and there are fixed coefficients (supposedly different in the two sectors, although Mrs. Robinson is very imprecise about the nature of the " single technique ")[3] between the outputs and the inputs of labour and machine use. This gives us a matrix of four technical coefficients a_{ij}, representing the amount of the j'th input required for the production of a unit quantity of the i'th output. We shall suppose the numbering to be such that the output suffixes 1, 2 stand for producer, consumer goods respectively, and the input suffixes 1, 2 for machines, labour.

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[1] Joan Robinson, " A Comment ", *Oxford Economic Papers*, vol. II (1959), p. 141.

[2] T. Barna in *Economic Journal*, vol. LXVII (1957), p. 490.

[3] Mr. Worswick cannot find any clear indication of the supposed nature of the capital goods sector, and Mrs. Robinson, in her comment, does not provide the answer. Worswick provides two slightly different models. *Op. cit.*

In addition to the technology, Mrs. Robinson supposes us to know the total number of machines, m,[1] , available for use at a given time, and the real wage rate in terms of the consumer good, w. The limitation on the availability of machines gives us our first boundary condition which, denoting the outputs of the producer and consumer goods sectors by x_1 and x_2 respectively, we can write as

$$a_{11}\, x_1 + a_{21}\, x_2 \leqq M \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

We have also the fundamental behaviour pattern of the society, that all wages are spent on consumer goods, and only wage earners buy consumer goods, so that the output of consumer goods (x_2) must be equal to the wages paid in both sectors,

$$w\,(a_{12}\, x_1 + a_{22}\, x_2) = x_2 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (2)$$

$$\text{or, } w \,.\, a_{12}\, x_1 - (1 - w \,.\, a_{22})\, x_2 = 0$$

If we take the equality sign in (1) (machines and not labour are the limiting factor) then (1) and (2) determine a unique pair of non-zero levels of output for the two sectors, provided that none of the technical coefficients are negative, not more than one is zero, $w \,.\, a_{22} \leqq 1$, and $a_{11}/a_{12} \neq a_{21}/a_{22}$. If the two latter ratios were equal, the producer and the consumer good would be identical (except for units of measurement) from the producer's point of view, and the solution would be indeterminate. The condition $w \,.\, a_{22} \leqq 1$ merely means that the product wage in the consumer goods industry must not exceed the average product per man.

Thus, if we assume full employment of machines, plus a perfectly elastic supply of labour at wage w sufficient for all requirements,[2] as Mrs. Robinson appears to do initially, then the model is a very simple one, with the outputs of the two sectors determined.

The profit in the consumer goods sector is, of course, determined —it is $(1 - w \,.\, a_{22})\, x_2$, but the profit in the producer goods sector cannot be found out from the model without some further assumption about behaviour, nor can the price of producer goods. Note that output and employment in the two sectors, and the rate of growth of the economy, are determined from the wage only, a point emphasised by Mrs. Robinson, and that the entrepreneur has virtually no function other than to refrain from buying consumer goods. With fixed coefficients, the entrepreneur has no decisions to make about factor proportions; he has constant unit costs and so merely produces as much as he can sell, and he automatically invests his profits in new machines since he never consumes, so that it is not surprising that we have not yet needed to introduce an entrepreneurial behaviour equation.

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[1] The machines must be capable of being used in the production of both producer and consumer goods, otherwise Mrs. Robinson's arguments do not hold.

[2] The labour used will, of course, be given by $L = a_{12}\, x_1 + a_{22}\, x_2$.

There is, however, one role left to the entrepreneur—he can choose whether to make producer goods or consumer goods, and both will only be produced if the profit per machine is the same in both sectors.[1] This behaviour pattern is assumed by Mrs. Robinson, and is sufficient to determine the price of producer goods. The profit per machine-year in the consumer goods industry is, of course, given by

$$\frac{1 - w.a_{22}}{a_{21}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

If P is the price of producer goods, the profit per machine-year in the producer goods sector is then given by

$$\frac{P - w.a_{12}}{a_{11}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (4)$$

and, since this must be equal to the expression in (3), we can determine P. This also determines the rate of return on investment, since we have the profit per machine-year, from (3), the price of a new machine, P, and the purely technical relationship as to the rate at which the machine can be used.

This is Mrs. Robinson's basic model—a simple two-sector model, completely determined once the wage rate is given, and a useful basis for further development. Mrs. Robinson has given only enough substance to the model to determine the *equilibrium* configuration, which is quite satisfactory if this is what one is after; but Mrs. Robinson is really after something more, which is the *process*, and for this she simply has not provided the materials, a fact that is abundantly clear when one attempts to set up any dynamic equations for the model.

So we have the worst faults of the " words and pictures "[2] school, the " realism " of which consists, not in attempting to discover what characteristics of the model additional to those necessary for equilibrium analysis we must have for process analysis, but merely in talking about a very highly simplified model as though it were a real economy.

This failure to perceive, as a mathematical approach would have made clear, that the model merely tells us what state of the economy is compatible with equilibrium, is clearly shown in Mrs. Robinson's chapter 8, and well illustrated by the following: " The proposition that the rate of profit is equal to the ratio of accumulation to the stock of capital (when no profit is consumed) cuts both ways. If they have no profit, the entrepreneurs cannot accumulate, and if they do not accumulate they have no profit ".[3]

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[1] " Entrepreneurs specialise on the production of particular commodities or items of capital equipment, but they are free to switch from one line to another so that a uniform rate of profit is established throughout the system ", *The Accumulation of Capital*, p. 74.

[2] Mrs. Robinson has, in fact, remarkably few pictures in this book, and these are segregated in an appendix, although one is thoughtfully provided as a bookmark.

[3] *Ibid.*, p. 76.

In the first place, the equality of profit[1] with accumulation is not a " proposition ", it is merely the restatement of the assumption that all profits are spent on producer goods, so that it does not " cut " either way. Even if we look at it as though it were an equilibrium condition, it merely means that profits and accumulation coexist at equilibrium. If we had a clear statement of process, then we would have to have specified what happened when the economy was not in equilibrium, and some causal sequence; it might then be possible to say either that accumulation at time $t + 1$ was zero because profits were zero at time t, or that profits at time $t + 1$ were zero because accumulation was zero at time t, but to make accumulation equal to lagged profit and profit to lagged accumulation locks the system rigid and denies the possibility of a dynamic process.[2] Thus Mrs. Robinson's proposition, even if it were a true proposition, could cut only one and never both ways.

Further on in the analysis, after having discussed the fact that simple growth equilibrium (in which the relations between the variables at any point in time are the same as the relationships at any other point, but the size of the growth variables changes with time) is possible if the labour force is growing at the same rate as the capital stock, Mrs. Robinson goes on to discuss the effect of a more rapid growth in the labour force than in the capital stock:

> The manner in which the situation develops depends upon how entrepreneurs react to the fall in (money) wages. If they maintain a constant rate of accumulation in physical terms, so that employment in the investment sector is unaffected, the wage bill in that sector, and consequently quasi-rents in the consumption sector, falls in the same ratio as the wage rate. In competitive conditions prices are reduced in the same proportion so that . . . the real wage remains constant.[3]

We have already seen that the model is so constrained that there is only one possible equilibrium wage rate associated with any given rate of accumulation (i.e. of output of producer goods), so that, as an equilibrium relationship, what Mrs. Robinson says is merely a statement of the obvious. As a description of a *process*, what she says is hopelessly inadequate.

Consider what does happen if, starting from equilibrium, there is a fall in the real wage w.[4] With nothing else yet having reacted to the change, there are three aspects of the situation to which reaction may occur:

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[1] Mrs. Robinson uses the term " profit " for output less wages less amortisation, and the term " quasi-rent " for output less wages.

[2] If $A_{t+1} = P_t$ and $P_{t+1} = A_t$, then $A_{t+1} = A_{t-1} = A_{t-3} \ldots\ldots$ Strictly speaking, this is satisfied not only by a single constant value of A, but also by a pair of constant values, each value appearing in alternate periods, but this oddity disappears if there are distributed lags.

[3] *Ibid.*, p. 79.

[4] Mrs. Robinson has the money wage rate fall, even though she has not, at this point in her analysis, introduced either money or any way in which money prices are determined. This does not matter very much, however, for if the change in money wages fails to change real wages even initially, then there is nothing to discuss.

(1) The wage rate having fallen, employment and output not yet having changed, the value of consumer goods coming on to the market is greater than the wages that are to be paid out.

(2) In the same way, the output of producer goods, at the previous prices, is less than the total profit that is currently accruing to entrepreneurs.

(3) Since the technical coefficients differ between the consumer and producer goods sectors, the fall in wages will change the profit per machine, at the previous relative prices, to a different degree in the two sectors,[1] so that, since these were equal in equilibrium, they are now becoming unequal.

To aspect (1), entrepreneurs may react by lowering consumer goods prices or by reducing consumer goods output or any combination of both, and these reactions may be rapid when they occur but delayed, or continuous but not rapid enough to keep up with the market situation, or at a rate depending on the size of the excess supply, or take any one of a multiple infinity of possible time forms. Furthermore, for the process to take place at all, there must exist stocks of excess commodities, no provision for which has yet been made in the model.

Again, with respect to (2), (and we must keep in mind that the rate at which profit is changing itself depends on the reactions to (1)), the entrepreneurs in the producer goods sector may react to excess demand by price or quantity or price and quantity variation, with all possible time forms. But the buyers in this market may react to price changes by investing in stocks (since we have had to allow them to do so) rather than machines, and these reactions may themselves take many forms in both extent and timing. And, of course, any change in the price of producer goods *vis-a-vis* consumer goods changes the rate of return on new investment (but not the profit per machine) and may have further effects if the entrepreneurs of our model are considered to make true investment decisions.[2]

The growing inequality in the profits per machine between the two sectors will give rise to further reactions, with entrepreneurs possibly moving from the low profit sector to the high profit sector, at a rate, and with such time lag, as is either specified for a model, or empirically determined for a real economy. The reaction here is particularly interesting since we cannot even know its *direction* without having specified the technical coefficients of the sectors.

Equilibrium and process are two distinct things from an analytical point of view, and it is equilibrium analysis to which Mrs. Robinson's methods and models are adapted. In the section of her book in which she has a more or less clearly defined equilibrium model, that of long-run accumulation, her discussions of processes consist either of reiteration of the equilibrium conditions, or of the arbitrary selection of one possible process from an infinity of possibilities, with no particular attempt to justify the selection. When she leaves the equilibrium model behind, as in her discussion of short-period movements, her work becomes narrative rather than analysis—but still she gives no reason as to why she should have chosen to tell one story about the ups and downs of the economy rather than another.

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[1] The profit per machine will rise more in the " labour-intensive " sector, that is, in the producer or consumer goods sector according as $\frac{a_{11}}{a_{12}} < \frac{a_{21}}{a_{22}}$ or $\frac{a_{21}}{a_{22}} < \frac{a_{11}}{a_{12}}$.

[2] Strictly speaking, this basic model has no place for such decisions, but Mrs. Robinson does write of the possibility that " it may happen that each entrepreneur individually is satisfied with the amount of capital that he has already accumulated " (*ibid.*, p. 76), which clearly implies decision about the desired amount of capital for the firm, and hence about investment.

The essential faults of method in the book are not those of Mrs. Robinson herself, but of the tradition in which she is working, the tradition of qualitative verbal analysis. (If one were to blame Mrs. Robinson personally it would be for being unable to resist trying to make so much out of comparatively little.) Now verbal analysis, assisted by diagrams, can be adequate for much equilibrium analysis involving only a few variables, and the lack of quantitative content can be handled by a taxonomic approach covering all eventualities. Such an approach may provide, in the absence of quantitative data, a way of finding answers rather than answers themselves, but it can demonstrate, and sometimes even prove, conditions of equilibrium. The moment we move away from the analysis of equilibrium situations, however, we are in a jungle through which we are only just beginning to learn how to find our way. Taxonomy becomes useless because the number of ways in which things may happen multiplies enormously in moving from equilibrium to process analysis.[1] Because taxonomy fails, so does any attempt to *analyse* a dynamic process in verbal terms; at best[2] one can *describe* verbally a process which has been traced out by other methods, just as we can describe the course of the planets around the sun without being able to show verbally why the course is as it is.

Mere plausibility in description can prove nothing about any actual process, if only because the number of ways in which a process could take place is so great that there is little difficulty in finding a possible path: the difficulty is to discover which of the possibles is the actual (or the relevant, in a theoretical model).[3] The traditional armchair empiricism is remarkably unhelpful in helping us to choose from among dynamic processes, since we need, more often than not, to know quantitative relationships and time shapes even to determine " qualitative " outcomes (i.e. directions but not magnitudes), such as whether a government policy is inflationary or deflationary.[1]

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[1] Compare the complex taxonomy of Samuelson's well known multiplier-accelerator model, one of the simplest possible completely defined processes, with one variable, a fixed, undistributed lag, and two parameters.

Vide P. A. Samuelson, " Interactions between the Mutiplier Analysis and the Principle of Acceleration ", *Review of Economic Statistics*, May 1939, reprinted in American Economic Association, *Readings in Business Cycle Theory*, 1944.

[2] For many purposes, such a description may be adequate, but a description can only be justified if it is of a process for which there is a publicly available analysis backing the description.

[3] Compare Samuelson's remarks: " The number of conceivable models (i.e. of a process) is literally infinite and a lifetime may be spent in exploring possibilities; furthermore, by supplying the proper stage directions at the proper time, we can specify any sort of a sequence development desired and may find that there is almost no empirical content in the theory being expounded ". P. A. Samuelson, " Dynamic Process Analysis " in Ellis (ed.), *A Survey of Contemporary Economics*, 1948, p. 375.

The relevant criticism of the verbal tradition in relation to Mrs. Robinson's book is not that it is limited, but that its adherents (Mrs. Robinson included) are not aware of its limitations, so that what is little more than a two-by-two linear equilibrium model (and well deserving of discussion as such) can be dressed in a woolly cloak of words to appear to be a full-blooded economy.

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[1] See A. W. Phillips, " Some Notes on the Estimation of Time-Forms of Reactions in Interdependent Dynamic Systems ", *Economica*, vol. XXIII (1956), pp. 99 103; also " Stabilisation Policy and the Time-Forms of Lagged Responses ", *The Economic Journal*, vol. LXVII (1957), p. 265.

Book Reviews

Lectures on Economic Principles. By Sir Dennis H. Robertson. Staples Press Ltd. Vol. I, 1957, 172 pp.; Vol. II, 1958, 162 pp.; Vol. III, 1959, 164 pp. 16s. per volume.

When it first became known that Sir Dennis Robertson was editing for publication the three-term course in economic principles which he had given at Cambridge since the war, some may have thought—as did the present writer—that this was a pity and that the reproduction of lectures, however distinguished, was inferior to their transformation into a full-blown treatise. But, now that publication is complete, it is clear—as we ought to have known—that Sir Dennis knew best. For, with characteristic stylistic insight, he has perceived that by preserving the lecture form he avoids the necessity of all sorts of technical apparatus of little intrinsic interest, while permitting himself the liberty of asides, inappropriate in a severer framework, which lend much greater piquancy to the treatment than would be possible in a treatise.

As a result he has produced a work which certainly kills two birds with one stone. It is an excellent introduction for beginners—not children, of course, but beginners of reasonably mature years, capable of treating economics as a serious subject. And at the same time it is a work which, like most of the best works on general principles, will yield its highest value only to those who are well conversant with the subject and are in a position to appreciate the nuances and periphrases with which the distinguished author makes subtle allusion to, or avoidance of, special theorems or difficulties. All of which must give it a special place among works of its kind. It is only necessary to compare any small sequence of paragraphs with a sequence of similar length in the more popular textbooks of the day to realize that here is a texture and weight of quite a different order. It has the quality of conversation that you feel *must* be written down.

For reasons which are not entirely apparent, it appears in three very slender volumes. Presumably there were some good reasons for this. But it may well militate against its use as a textbook; and it is, therefore, very much to be hoped that in future editions it will appear between two covers only—it would still run to only just less than 500 pages. What is much less easy to understand is why a reputable firm like Staples should issue these volumes with spines which are not uniform. Vol. I has a simple asterisk to denote its place in the series and the Staples shield or arms at the bottom; Vol. II, " Vol. II " instead of two asterisks, plus the shield or arms; Vol. III, " Vol. III " and no shield or arms. Has ever so important a work in economics been so casually treated ? The only consolation I can think of is that in years to come this blemish may give this edition some slight extra value for the more devout of the race of collectors. " ROBERTSON, D. H., LECTURES ON ECONOMIC PRINCIPLES, first edition, three volumes, in prime condition *with the non-uniform spines* "—thus we may conceive the antiquarian booksellers' catalogues of the year 2060.

The contents are divided according to the volumes. Vol. I deals with preliminary notions and the theory of price—demand and utility, factor combination, costs, competition and monopoly, joint supply and price discrimination, etc.; Vol. II with distribution in the sense of the pricing of the factors of production—demand and productivity, rent, capital and interest, profits and the demand for and supply of labour; Vol. III with money and fluctuation—monetary equilibrium, banking, stagnation theory, money and interest, the trade cycle and trade cycle policy. Throughout all this the treatment is enlivened by illustration from contemporary events and side glances at problems of contemporary policy.

It will be seen from what has just been said that these lectures stand deeply rooted in the Marshallian tradition of arrangement, and a very brief glance at the substance will show that this influence is not limited to arrangement. Even in Vol. III, which is largely built on Sir Dennis's own contribution, there can be detected some continuity with the mood, if not the detailed technique, of *Money Credit and Commerce*, still more with the earlier and more virile evidence before the Gold and Silver Commission and the Committee on Indian Currency. I do not know what Marshall, whose sense of humour often seems to me to have been somewhere in the neighbourhood of absolute zero, would have thought of some of Sir Dennis's more playful witticisms, particularly of the drawing depicting a forlorn and self-abasing " Rent " standing forever debarred from " entering into " a magnificent Greco-Roman edifice labelled " Cost of Production." But I suspect that on a calm survey of his intellectual progeny he would have found more of his own instinct for reality and conception of the rôle of theory, and a more faithful use of his own particular methods, in Sir Dennis than in any other.

It would be a great mistake, however, to regard these lectures as merely a translation of, or variations upon, Marshallian themes. It is true that the main drift of the argument in Vols. I and II tends on the whole to Marshallian conclusions. Measurable utility, consumer surplus, the time element in the determination of normal values, marginal productivity and the importance of the supply side in long-run distribution theory—these are indeed all there. But the exposition is intensely personal and is enlivened throughout by critical sallies and incorporation of recent improvements. Sir Dennis is very much in touch with a world-wide profession; and although, outside those spheres of Vol. III which are especially his own, he tends to conservatism in his choice of techniques, he is well aware of other usages; and where they lead to results bearing on the main corpus of principles as he conceives them, he contrives, either by way of critical rejection or acceptance and assimilation, to keep his readers in touch. The exposition of interest theory in Vol. II with its borrowings from Lerner, Ramsey and Champernowne is an especially felicitous example of this process of judicious modernization.

As for Vol. III, of course it is predominantly original. Much of it is based upon earlier work by its author. But nowhere has he provided so complete a synthesis. And now that it can be surveyed in this form, it is possible to see more clearly than ever before his achievement in this sphere. I mentioned *Money Credit and Commerce* to draw attention to background and continuity. But it is hardly an exaggeration to say that the difference between that work and these lectures represents a substantial part of the progress of thought since Marshall in the subjects with which they deal. Doubtless not all: there have been, and there are, other important participants and other important contributions. Nor can it be said that all the progress is as yet consolidated: this part of the lectures is much more occupied with controversy than the rest. We do not agree about all these matters yet; and very important issues of policy are involved. Yet I submit that no one, reading this part of Sir Dennis's book, could seriously argue that we were not now far nearer to a proper understanding than we were thirty years ago. And no one with even a speck of justice in his make-up could deny to Sir Dennis a very appreciable share of the credit.

It would be inappropriate in a short notice of a work like this even to begin critical comment on matters of detail: that can wait for different contexts in which specific references are appropriate. But it is perhaps not out of place to enquire concerning the general impression. How does the Marshallian tradition, as embodied and developed in this work, commend itself at the present day as a mode of approach and a method of analysis? Many of the developments of the last seventy years have occurred by way of criticising Marshall and his intellectual habits; so many of us have been brought up, as it were, on running Marshall-critiques. How does it seem when restated and developed from Sir Dennis's point of view? How does it survive the acid of contemporary doubt?

In my judgment, well. Let me hasten to say that by this I do not wish to express complete agreement with all the positions adopted: I have broken friendly lances with Sir Dennis in the past and I hope and expect to do so in the future. Nor do I wish to seem unaware of the various strictures that could be passed on grounds of insufficient rigour. It is quite true that this kind of approach is, so to speak, *quasi-literary* in manner. It does not set out its analysis in the form of strict mathematical deductions. It makes no attempt at exhaustive definitions and classifications. It would be wrong to say that it proceeds by instinct; for that would imply a kind of a-scientific mystique. But it would not be unfair to say that, in contrast to the more rigorous and abstract systems, it proceeds by way of commonsense appraisal of relevance, of a selection of simplifying assumptions to keep the argument down to earth and of a determination throughout to judge each section of analysis, not only by its internal consistency but also in relation to a general perspective. It is a bit of an art as well as a science. But, for all that, and also in contrast to many more abstract systems, it does deliver the goods. If we are honest with ourselves we have to admit that when we condescend to talk about the real world, then, however pure and high-falutin our analytical fancy, in a great many cases at least, we have to adopt something like these simplifications. And if we are considering the matter as teachers rather than pure research workers, I am pretty clear that a training which begins from this kind of basis is much more likely eventually to produce what we should call an economist than one which begins with all sorts of bits and pieces of more severe techniques which, whatever be their promise for the future, have not as yet found their pedagogic bearings. I am clear, too, that it will be much more of a general liberal education. A man will learn more of the real world from this kind of theory than from any number of geometrical exercises; and he will learn, too, habits of thought which will help the establishment of a proper perspective if he considers the world in order to run it or to change it.

There is one gap in this admirable work. Because the Cambridge academic year only extends to three terms, the course on which it is based did not include anything on international economics. Down to earth as it is in many respects, it is still the economics of a closed community. Surely we must all beg Sir Dennis to make good the deficiency. And if it be the case that inspiration in this connection only comes with the compulsion to lecture, surely there must be many universities in the English-speaking world who would be willing to provide the stimulus. Please, Sir Dennis, make haste to complete the series in this way—even if at first it involves still less uniformity in the spines.*

ROBBINS.

Linear Programming and Economic Analysis. By ROBERT DORFMAN, PAUL A. SAMUELSON and ROBERT M. SOLOW. McGraw-Hill Publishing Co. Ltd. 1958. ix + 527 pp. 77s. 6d.

If this book had been written single-handedly by any one of its authors, most economists would have expected it to be a work of outstanding excellence; but when all three of them combine, nothing short of a *tour de force* would fulfil expectations. Furthermore, Professor Baumol's statement on the dust jacket that " it is a magnificent accomplishment ", adds to its credentials to an extent which seems to make a review wholly superfluous. Indeed, even were a reviewer to restrict himself entirely to laudatory comment, he would still feel himself to be in danger of *lèse majesté*. The fact is, nevertheless, that certain critical remarks are in order, partly in connection with the expository quality of certain parts of the book, and partly in connection with the standpoint from which the authors have approached their subject.

The book itself may be divided into three parts. The first eight chapters are on programming, linear and non-linear, and its application to the theory of the firm. The next six chapters are devoted to various general equilibrium models, both static and dynamic. Finally, there are two chapters and an appendix dealing with certain elementary problems in the theory of games. (There is also an appendix entitled, " The Algebra of Matrices ".)

Applying the criteria of rigour, clarity, and interest, I consider the central six chapters to be one of the finest pieces of writing of its kind I have seen in economics. The opening eight chapters I rate as very good, but occasionally dull, repetitive, and unnecessarily difficult to follow. The chapters on games are also good, but inferior as introductory expositions to either the Kuhn lectures or Mckinsey's book.

To take the best part first. Chapters 9 and 10 contain a full and clear account of the theory of the Leontief system with comprehensible explanations of such hitherto recherché matters as the Hawkins-Simon conditions, and the Samuelson substitution theorem. The next two chapters deal with the dynamics of general equilibrium models, and questions of efficient growth and capital accumulation. Non-advanced mathematical economists who have fought shy of the multifarious papers in *Econometrica* relating to these topics may again be put off by the amount of mathematical symbolism used; but this would be unfortunate. Although they are not light reading, it is not necessary to be a Dorfman, a Samuelson, or a Solow to work through them and obtain a wider and deeper understanding of some of the latest contributions to mathematical economics. Lastly, there are two chapters on general competitive equilibrium and its relation to welfare economics. Here, in addition to brilliant descriptions of the Walras model and some

* Note: Sir Dennis asks me to say that on page 99, line 21, Vol. III, " OF " shoud read " OR ", and on page 107, line 5 from the end, " simplest exposition " should be " simpliste expositions ".

of its variants, and of the von Neumann model, the authors take up the question of proving the existence of solutions. They provide the requisite proofs, explain the assumptions which need to be made, and go on to produce a convincing justification why a proof of existence is desirable, anyway. The only flaw in all this is in their discussion (pp. 353–354) of the counting of equations and the distinction between relative price and absolute price determination which the reader may find a trifle confusing.

The first section of the book offers arithmetic, geometry, and algebra plus intuitive economic argument as a means to understanding linear programming. There is no shortage of examples ranging from the standard one of the optimum diet through international trade to the theory of the firm. In fact, there is a slight repetitiveness which is likely to bore the reader and put him off a little, much as he will gain by shirking nothing and working through every example. I think he may also become a trifle worried by the tendency of the authors to produce their conclusions as they go along. At the end he may not find it at all easy to obtain an overall view of the subject, especially if he omits chapter 4, " The Algebra of Linear Programming ".

On the theory of the firm two comments must be made. The case of increasing marginal cost is dealt with by postulating the existence of activities each with specific upper limits to their level of operation. This is an unnecessary complication because in the usual linear programming problem marginal and average cost will be increasing as long as there are any variable costs. In addition, monopoly and imperfect competition are mentioned under the heading of non-linear programming when at least some elementary problems in this field can be analysed by means of linear programming, the only cost being to make the models rather more elaborate. Since one of the authors is undoubtedly aware of these points, and since they are of considerable interest to students of economics, it is intriguing to wonder why they were left out.

The remainder of the book, on games, is not bad as far as it goes, and the authors themselves refer the reader to other literature which will enable him to progress further in the subject. The appendix on measurable utility provides a simple explanation of this topic using the " strong independence axiom ". It is interesting to note that Dorfman, Samuelson, and Solow are not optimistic about the use of games theory in economics. A future historian of thought may cherish the quotation: " Economic problems, and particularly the interactions of the economic objectives of diverse individuals, are enormously complex; one would hardly expect to reduce them to the level of parlour games, even theoretically ". It is impossible to resist asking the question: " What then can be expected from the traditional approach to micro-theory ? "

To turn finally to that fashionable pursuit, methodology, the obvious weakness of the book is that little or no attempt is made to explain how linear programming has been, is, or will be productive of positive, testable hypotheses in economics. The emphasis in the title is on analysis, and this seems to be somewhat misplaced for a technique which is so clearly susceptible of quantitative application. The significance of the Leontief system is undoubtedly to be assessed in terms of its empirical applicability, and the same must surely hold true for other linear models.

In conclusion, it is superfluous to say that this is a book which every economist ought to read. It is also a book which any undergraduate specialising in economics could use. The mathematical ability required for much of its contents does not go beyond the level required for the old General School Certificate, and any student who reached that level could go on to master the more advanced sections. Certainly, it cannot be read through at a sitting, but this can hardly be a *sine qua non* for any textbook.

M. H. PESTON.

Water Resource Development. By OTTO ECKSTEIN. Harvard University Press. Oxford University Press. 1958. xvi + 300 pp. 52s. *Multiple Purpose River Development.* By JOHN V. KRUTILLA and OTTO ECKSTEIN. The Johns Hopkins Press, Baltimore. Oxford University Press. 1958. xvi + 301 pp. 36s. *Efficiency in Government through Systems Analysis, with Emphasis on Water Resource Development.* By ROWLAND N. MCKEAN. John Wiley & Sons Inc., New York. Chapman & Hall Ltd. 1958. xii + 336 pp. 64s.

All three of these books deal with the economic problems of investment projects which yield benefits in the form of hydro-electric power, irrigation, flood control, land drainage, navigation or some combination of these things. The three are far from being substitutes, however. McKean's is perhaps addressed to the widest audience, for he is endeavouring to persuade the reader that operational research methods can improve government budgeting. His book therefore starts with an account of the theory of choice—the principles of rational decision making—which is wholly admirable. Something very like it ought, in the reviewer's opinion, to constitute a part of every introductory course in economics. The irrelevance of sunk costs, the choice of appropriate alternatives, the marginal approach, the dangers of sub-optimisation, the problems of choosing the right maximand, the discounting of future costs and receipts and such matters are all explained clearly but without boring formalism.

McKean next turns to the problems of computing and comparing the costs and benefits of water resource projects in order to provide a basis for the choice of projects. Then the third part of his book provides case studies of some examples. These two parts cover much the same ground as the bulk of Eckstein's book, though the arrangement is different. Eckstein has three general chapters on benefit-cost calculations followed by separate discussions of flood control, navigation, irrigation and electric power which intermingle discussion of principle with illustrative examples. Unlike McKean, Eckstein also provides some discussion of the problems of financing public projects. In addition, Eckstein has contributed a chapter to Krutilla's book where he calculates the opportunity cost of funds raised by taxation in the U.S. in 1955. He summarises this ingenious and elaborate statistical exercise in his own book as follows:

For two alternative assumed " tax changes, a distribution between large and small businesses and between households in different income classes was worked out. The effects of the taxes on investment were estimated for small and large businesses, and empirically derived rates of return were assigned. Each income class of households was divided into subgroups, according to the interest rates at which they made their lending-borrowing decisions. To estimate the interest rate applicable to any particular set of tax changes, a weighted average of the relevant interest rates and rates of return was taken, with the weights determined by the distribution of incidence of the taxes among businesses and among households ".

Krutilla's book is primarily devoted to the use rather than to the construction of cost-benefit estimates. Part I briefly justifies the use of such estimates, explains why indivisibilities, external economics and non-marketability often necessitate some form of public action in respect of water resources in order to achieve efficient resource allocation and, finally, presents Eckstein's estimate that in 1955 the social cost of capital was between 5 and 6 per cent. Part II consists of three case studies of particular river projects. The first compares the efficiency of two alternative private and public projects and shows why a third, more efficient scheme would not pay a private developer. The second case is one where " all the necessary conditions, both physical and institutional, for the development of a river basin in the most efficient manner under private auspices . . . are present ". The third case study is of a project where the choice between public and private enterprise must primarily depend upon evaluation of their effects upon income distribution. These effects are estimated and compared for three alternative ways of financing the investment. It is a commonplace that matters of economic policy must be decided by examining their distributional aspects as well as their efficiency aspects, but very few writers have ever got down to the hard job of working out the actual numbers in a real problem. Krutilla has done it here, however, making a most valuable contribution.

The authors do not always agree among themselves. One disagreement is particularly revealing, namely, McKean's and Eckstein's different views on the selection of the discount rate to be applied to future benefits and costs when calculating their present values for the purpose of selecting the best set of projects within a limited budget. (For a discussion of their different opinions and a third view, see pp. 102–7 of Margolis' review of the three books in *American Economic Review*, March 1959.) Suppose, for instance, that the choice lies between two projects only, both requiring the same initial outlay, and that their future benefits and maintenance costs are all expressible in monetary terms and known with complete accuracy and certainty (so that most of the other problems discussed are assumed away if, in addition, their distributional effects are ignored). Suppose, finally, that the present value of future benefits less costs of project A is greater than B's at interest rates above 5 per cent. and less at interest rates below 5 per cent. Which project should be chosen, A or B ?

The answer is far from obvious, since several considerations are relevant. One, which involves a value judgment, is the time preference of the political decision-taker. Another is the opportunity cost of the funds which will be required in the future to defray project maintenance costs or alternatively the yield which will be obtained on reinvestment of any net future revenues which accrue to the investing authority. The problem is an interesting one and demands further thought.

All three books contain a demonstration that, given the usual conditions, competition produces a Paretian optimum. In each case this follows familiar and dull lines, and is then used either to justify public intervention in the water resources field or to justify the use of cost-benefit analysis. Strictly speaking, however, all that can be shown along these lines is that if the welfare conditions are *all* fulfilled *everywhere* else, the government should intervene in the water resource field, designing its intervention by using cost-benefit analysis. But it does not automatically follow that these conclusions also hold for the second-best world we live in.

Eckstein and McKean are both aware of the problem, but devote far too little space to it. Eckstein declares that he will " take the position that under conditions of high employment prices are valid indicators of social benefits and costs, as the (pure welfare theory) model suggests, unless there are specific circumstances in the situation which require some modification of the analysis ". He justifies this by maintaining that divergences between price and marginal cost are " widely—also perhaps evenly—diffused and relatively small, particularly in the range of markets most relevant to water-resource development ". This may be true, but it deserves more than two sentences and a footnote reference to Little's book on fuel policy as supporting evidence !

McKean suggests that " if a frequency distribution of the possibilities is imagined it seems likely that increased production where price exceeds cost would usually be a step toward efficiency ". This is an interesting approach, but it demands elaboration.

However inadequate these justifications may be in principle, most economists concerned with practical matters would agree with the authors that cost-benefit analysis is better than, say, tossing a coin. All one can do in practice is to look out for palpable divergences between price and marginal cost which directly affect cost and benefit estimates. Some nice examples are given; here, for instance, are three discussed by Eckstein:

(i) The cost of land acquired for a project frequently implies a capitalisation rate widely different from that used in capitalising future benefits and maintenance costs of the project.

(ii) The opportunity cost of funds raised by taxation differs substantially from the interest rate on government borrowing.

(iii) Railway rates often exceed marginal cost, so that the saving to shippers resulting from a new alternative mode of transport exceeds the saving to society.

It should be clear from what has been said that these books are of much wider interest than one might think at first glance. Problems of estimating benefits and costs in order to select investment projects and to see whether private and public profitability coincide are problems which occur in many contexts. (National parks, the Channel tunnel, road programmes and urban reconstruction are obvious examples.) These contributions to applied welfare economics therefore deserve attention and imitation in this country.

RALPH TURVEY.

The Theory of Wage Determination: Proceedings of a Conference held by the International Economic Association. Edited by JOHN T. DUNLOP. Macmillan & Co. Ltd. 1957. 437 pp. 36s.

This is one of the most stimulating of the volumes which have emerged from the annual meetings of the International Economic Association, even though, or perhaps because, the conclusions on its subject-matter are so elusive. The theory of wage determination is clearly one of the least satisfactory portions of economic theory, perhaps because it is less exclusively economic than many others, and as is only proper the contributions to this volume attack the problem from a number of widely different angles.

Professor Dunlop leads appropriately with a survey of the question which ends in some econometric results of a delightfully perverse and frustrating character. The next section is that where the greatest difficulties are met with since it deals with the theory of the general level of wages and by implication the theory of the share of wages in the national product. Professor Harry Johnson opens with a theoretical contribution which sets out briefly the differences between Keynesian and classical theory, and the limitations which have emerged from attempts to develop the former. Professor Gasparini follows a somewhat similar plan, setting out the models in question more formally and referring to the econometric works of Klein and Tinbergen. The really firm appeal to experience, however, is left to Professor Phelps Brown who provides a most stimulating synthesis of his previous articles on the share of wages in the British national income, attempting to explain qualitatively rather than quantitatively the main shifts which have been perceptible in factor shares over the last seventy or eighty years. Professor Bent Hansen returns to the field of pure theory with an account of the determination of the wage level which, like some of the crucial passages in his book on the theory of inflation, seems to make some rather daring assumptions about the possibility of drawing a general demand curve for labour. Professor Wagner concentrates mainly upon the analysis of Swiss experience under full employment in which, however, it is not clear that sufficient attention is paid to import prices as an explanation of what actually happened to the internal price level. Professor Krelle sets up the most ambitious theoretical model in this collection, though it is one which he does not attempt to quantify except by making assumptions about the broad ranges within which some of his key variables must lie, and like most models of this nature it can hardly claim to be more than one of an indefinite number of possible theoretical constructions which might satisfy the requirements equally well.

In the third section we come to a more institutional and historical examination of the impact of the labour union on the development of wages. Mr. B. C. Roberts and Mr. H. A. Turner deal with British experience, Professors Brochier and Marchal with French. This part runs naturally into the succeeding one in which an examination is made of wage structures by Professors Kerr and Reynolds with special reference to the United States, Dr. Rehn for Sweden, M. Rottier for the United Kingdom and Professors Perroux and Sellier for France. In the fifth section the theory of bargaining is brought to bear upon wage determination by Dr. Rothchild and Professor Shackle, the former discussing the problem in somewhat general terms, the latter setting out the machinery of a much more formal analysis. The final section deals with the supply of labour. Professor Charles Myers surveys American experience and Professor Demaria sets out a brief theoretical analysis based on the usual neo-classical premises. The report of the proceedings which followed the reading of the papers is interesting but like most such records of a discussion on a subject of such complexity is bound to leave the reader feeling that it is a quarry in which to search for stimulating suggestions rather than an orderly progress between firm conclusions.

The book as a whole can certainly be regarded as providing a good sample of the current and recent lines of attack upon problems of wage determination. It shows economists at their not unusual activity of worrying at a set of intractable problems from which the reader may feel that they do not extract a great deal of nutrition. Economists as a race have, however, long been accustomed to deriving more stimulation from the absence of agreed and demonstrable conclusions than from their presence, and the state in which this collection shows the theory of wage determination to be will not make them unduly despondent.

A. J. Brown.

Trade Unions in a Free Society. By B. C. Roberts. Institute of Economic Affairs. The Stellar Press Ltd. 1959. 120 pp. 9s. 6d.

Unions in America. By B. C. Roberts. Industrial Relations Section, Princeton University, New Jersey. 1959. x + 136 pp. $2.

These two books have much in common. They both discuss the problems arising from the existence of trade unions in a democratic society; the first, in relation to Great Britain, the second, to the United States. Both discuss many of the same topics, but in different environments and addressed to different audiences. *Trade Unions in a Free Society* is part of a series of publications designed to examine " economic and social policies by reference to the requirements of a free society ", whilst *Unions in America* is an appraisal of trends from a British standpoint.

The great growth of trade unions during this century has raised fundamental problems for society. The author assumes that the individual's freedom to organise in support of his own interests is a liberty essential to a democratic society that should only be limited by the right of others to do likewise. Power in a democratic society is thus " diffused through a multiplicity of agencies " and the inevitable conflicts between them are settled not by central authority but by " an interplay of social and political forces ".

This sort of democracy may work reasonably well so long as the organisations are small and represent narrowly defined interests; but this is no longer true of trade unions. Today in Great Britain nearly one out of every two employees is a member of a trade union. When this degree of organisation is reached, problems inherent in the system of group pressures make themselves felt. In the first place, actions in support of sectional interests may dictate the policies of society as a whole. For example, pressure for wage increases by un-coordinated groups may accentuate an inflation that not only nullifies higher wages, but also damages the economy as a whole. Secondly, as unions grow and society becomes more intricately organised, the damage a strike may inflict on society becomes greater, and there arises the problem of the right to strike. Lastly, there arise a whole series of problems connected with democracy itself. How far should unions be allowed to use their funds to support a political party ? And, coming closer home, how far are unions themselves democratic ? Are they governed by a minority caucus ? And, are they tyrannical towards individuals, or do they respect the inherent democratic right to dissent from majority decisions ?

These fundamental problems are examined in *Trade Unions in a Free Society* in chapters dealing with the problem of wage inflation, the right to strike, compulsory arbitration, trade unions and politics, the problem of internal democracy, freedom of association and compulsory unionism. The whole forms a first-class essay: well balanced, moderate in viewpoint, but shirking no vital issue. Unfortunately, it is all too short and only covers some 50 out of the 120 pages in the book. The rest consists of appendices of very unequal worth. One of these, twenty-three pages long, a Chronology of Events connected with labour in Britain from 1349 to the present day, lacks historical sense of the relative importance of events and departs from chronology to express highly tendentious opinions upon them. It is out of keeping with the rest of the book and would have been better omitted.

Turning to America, the author emphasises the great extent to which democracy depends on " a deep respect for its rules and conventions ". However, American democracy often turns out to have little respect for conventions and becomes a conflict for power in which " only the roughest edges of the struggle are smoothed by the rules ". This is nowhere more conspicuous than in the internal democracy of the unions. Union leaders often achieve their position by all kinds of undemocratic methods of crushing opposition and rigging elections which give no opportunity for genuine choice by the members. Once power is achieved, it is used ruthlessly to oust all opposition and to appoint subservient committees and officials. Corruption is widespread and union funds are often used for personal ends.

The same crude struggle is in evidence in relations between employers and unions. Here, however, the " roughest edges " have been smoothed by the law which compels recognition and bargaining and outlaws some of the worst unfair practices. But, as the author remarks, reliance on the law is no substitute for self-regulation, and unless the laws are so framed that they encourage the growth of genuine democracy and respect for the rights of others, no progress will be made.

Mr. Roberts writes very well and his grasp of fundamentals is never obscured by detail. One wishes the two works could be combined together, for they would then make a first-class study in comparative industrial relations and the position of trade unions in the world's two chief democracies.

A. E. C. HARE.

The Economics of Collective Bargaining. By P. FORD. Basil Blackwell. 1958. xiv + 121 pp. 10s. 6d.

In this brief but substantial work Professor Ford draws on his unrivalled knowledge of the Parliamentary Papers to illustrate the workings of the British labour market in this century, and give the life and authenticity of the detailed instance to processes and forces that the text-books draw only in outline. About a third of the space is devoted first to some features of collective agreements and bargaining, and—a specially instructive section—the conventions under which this bargaining is carried out. The remainder comprises three main topics—the sources from which wage rises can be obtained; the principles on which wage awards have been based; and possible wage policies under full employment. There are some passages here where the discussion is conducted in abstract terms, but the most original contribution is made by those which bring together instance after actual instance for the evaluation of some general consideration—the section, for instance, that discusses the possibility of trade union pressure raising industrial efficiency, in the light of the Reid Report on Coal, the Report of the Working Party on Light Clothing, the Platt Mission, and the Herbert Report on the Electricity Supply Industry; or again, though this is an illumination of practice rather than theory, the section that makes clear what parties mean when they demand that a claim shall be treated " on its merits ", by bringing together four instances of that demand being made. Perhaps the title of the book itself, and some of its sub-titles, suggest more generality than the treatment provides; a comparative newcomer to the subject will hardly find an outline to fix in his mind, either of British procedure or of the economic analysis of wages. But the student who has a grounding in these things can widen his knowledge and deepen his understanding, in this harvest of experience of the institutions and forces of the British labour market. Nor will anyone wish to set out his own findings on any matter this study deals with, until he has followed up the references to which Professor Ford is a uniquely well-informed guide.

E. H. PHELPS BROWN.

The Strategy of Economic Development. By ALBERT O. HIRSCHMAN. Yale University Press, New Haven. 1958. xiii + 217 pp. $4·50.

If judged by the criterion of the frequency of challenging ideas, Professor Hirschman's book is a notable contribution to the study of economic development in underdeveloped economies. Even though some of these ideas call for qualification, or even for rejection, the work as a whole is a substantial and stimulating contribution to the subject. (It is fair to add that the author himself frequently makes the necessary qualifications, though sometimes these seriously restrict the applicability of the argument; and on at least one occasion (p. 196), an important conclusion depends upon an absent limiting assumption.) One's appreciation of the work is increased by its brevity; it is as if the author has deliberately confined himself to topics on which he has something interesting and enlightening to say—a commendable and unusual practice in a field which encourages verbosity and attempts at encyclopædic comprehensiveness.

Professor Hirschman has had extensive experience as economic adviser and consultant in Columbia. He explains that the writing of his book is primarily " an attempt to elucidate my own immediate experience in one of the so-called underdeveloped countries ", and his object is to provide new ways of thinking which will be helpful " to the decision-maker in underdeveloped countries when he has to determine basic strategic issues in development planning, such as the assigning of sector or area priorities or the kind of industrialization to be pursued " (p. vii).

His central approach is that economic growth, especially in underdeveloped countries, is an unbalanced process; that the emergence of lack of balance or disequilibria can be fruitful for further growth; and that official policy should at times set out deliberately to create unbalanced situations in order to induce adaptive growth. This thesis is complemented by another—that the " ability to invest " is lacking in many underdeveloped countries, even when there is latent entrepreneurial and management talent as well as savings. (" Additions to savings depend far more on the opening up of investment opportunities and on the removal of various obstacles to investment activity than on increased income " (p. 32).) The corollary of the second thesis is that " we must call forth as much decision-making ability as possible by maximizing induced or routinized decision-making " (p. 28). The contrivance of unbalanced situations is one way in which adaptive entrepreneurial responses may be induced and decision-making simplified. The discussion is not limited to adaptive responses initiated in the market;

for " it is our contention that nonmarket forces are not necessarily less automatic ' than market forces " (p. 63).

These theses and their implications are explored with elaboration and ingenuity. And, from his selected vantage-point, the author looks, often with fresh eyes, at such traditional themes as growth models, the balance of payments, foreign aid, population pressures, " social overhead capital *versus* directly productive investment ", and external economies and diseconomies; and particularly withering looks are cast on the theory of balanced growth, concluding, very relevantly, with the words: " We must recognize that there are tasks that simply exceed the capabilities of a society, no matter to whom they are being entrusted. Balanced growth in the sense of simultaneous multiple development would seem to be one of them " (p. 54).

Induced decision-making is particularly well discussed in chapter 8. (By contrast, chapter 6 on " inter-dependence and industrialization " is ineffective and unduly question-begging.) In chapter 8 the author is interested, *inter alia*, in identifying types of economic activity in which, once an undertaking has been established, the subsequent efficient direction and control of operations are rendered more or less compulsory or automatic, for example, where operations in the nature of things must be carried out in compliance with certain standards if at all (maintenance of aircraft *versus* maintenance of roads), or where they can be carried out without heavy strains on management. He is surely right in emphasising practical considerations of business administration which are often neglected; for " the ability to *promote* new ventures may outstrip the ability to *run* them efficiently " (p. 139). But one of his examples is questionable. " Under certain circumstances, it may be rational for governments in underdeveloped countries to concentrate on ' show pieces ': when a government undertakes the construction of a large hydroelectric station or of a steel mill, it simply cannot afford to let such ventures go wrong . . . " (p. 144). This ignores the fact—unless it is subsumed in the opening qualification—that governments are often able and willing to use their powers to make uneconomic and poorly organised undertakings appear to be " successful ": striving after maximum efficiency by careful planning and skilful management may not be the easiest way of appearing not to go wrong.

I hope the preceding outline of the main themes of the book establishes the freshness of the author's approach; as I have said earlier, the book is stimulating, even where the argument is not conclusive. There is, however, little point in listing a selection of the least acceptable conclusions, or in discussing some of the instances where the author's ingenuity or pursuit of paradox seems to have been overdone. This would give an unbalanced impression, unfair to the book.

B. S. Yamey.

The Strutts and the Arkwrights, 1758–1830. By R. S. Fitton and A. P. Wadsworth. Manchester University Press. 1958. xii + 361 pp. 35s.

This well-produced book can be regarded as a companion volume to *Samuel Oldknow and the Arkwrights*, published by the same press as the first of its economic history series in 1924. Richard Arkwright, although a central figure in both stories, still remains " a biographical enigma "; and, in the same way, the records available to the authors prevent them from adding much to the accepted pattern of the early cotton industry. The value of the book lies in the study it makes of one of the great industrial families of the late eighteenth century, and of its synthesis of existing evidence on Arkwright and the early factory system.

Jedediah Strutt, the founder of the family, was the son of a Derbyshire farmer, and as such was born in the centre of " hosiery country ". Trained as a wheelwright, he might be thought to have some knowledge of mechanics; and this, together with an innate ability and an acquaintance with the local industry, enabled him to perfect a machine for the knitting of ribbed stockings. Unlike many others, he was able to use his invention to advantage, and by the time of his meeting with Arkwright in 1769 was already a man of means. In taking up cotton spinning, Strutt switched his resources from hosiery manufacture to the production of one of its major raw materials; and by so doing, became one of the few successful innovators who supported another and a greater. Both he and his family moved in the close circle of the radical and Socinian branch of dissent, and it was from this group that he found his nearest business associates. Strutt thus differed in many ways from what might be called the typical cotton magnate of the time: and it would be wrong to think of him as an entrepreneur of heroic stature —he owed too much to favourable circumstances. Much of the credit for the great concerns at Belper and Milford must go to his three able sons. The eldest, William, an outstanding engineer and fellow of the Royal Society, erected the first multi-storey fire-resistant factory by the use of iron-framed buildings. And all three, by their liberality, contributed greatly to the amenities of Derby, making the town, with Birmingham and Liverpool, one of the chief centres of Unitarian culture, then at its brightest.

The second half of the book illustrates again the difficulty of generalization about conditions and standards of living of the early factory workers. The distinction which must be made between the establishments of the richer owners and those of the smaller men is reinforced. Belper, which fell into the first of these categories, was a self-contained factory settlement employing 600 persons in 1789, 1,200 in 1802, and 1,800 in 1833. As a creation of its time, it shared many of the characteristic features of these communities. Large numbers of children were employed (the Strutts opposed the early Factory Acts); indiscipline at work was controlled by fines: there was a considerable labour turnover; and most of what was earned at the mill returned to the employers by way of payments for rent, coal and food. Yet, there was much to be put to the credit of the employers. The company houses won the approbation of all who visited Belper; extensive provision was made for the education of the young; and truck seems to have been practised without protest. If the supply of some 700,000 gallons of milk between 1807 and 1842 at a profit of £235 is any standard, absence of unrest is not surprising. The picture of conditions is certainly not complete. One would, for example have liked the structure of family employment to be more fully analysed and to know what happened during the depressions in the cotton trade. Nevertheless, enough evidence is adduced to show that the Strutts were, by contemporary standards, good employers.

The conclusion that " the widespread notion that the pioneers of the Industrial Revolution were interested only in getting rich quickly will not bear scrutiny " is perhaps too sweeping. The Strutts represent eighteenth century paternalism and philanthropy at its best and not all were equally enlightened. On the other hand, the importance of this element in the conditions of the time has been greatly underestimated. Robert Owen's New Lanark Mills were far from being a unique phenomenon.

A. H. JOHN.

The English Utilitarians and India. By ERIC STOKES. The Clarendon Press, Oxford. 1958. xvi + 350 pp. 45s.

This erudite and interesting study by Professor Stokes is concerned with the practical as opposed to doctrinal influence of Utilitarianism. In its native breeding ground rigid " Benthamism " was soon worked into a much more flexible system of thought, mainly by the son, John Stuart Mill, of one of its founder-members and primary advocates. The dilution was such that in the field of political economy for example, it is not very easy to point to any specific Utilitarian influence. But Dr. Stokes' investigations attempt to show the doctrine in a new setting—the administration of Indian affairs. For a variety of reasons, among which Bentham's interest and James Mill's key position at the East India Company were perhaps the most important, Utilitarianism was introduced as a practical system to be applied to the government and administration of British India. A continuous flow of advice and memoranda from the pens of Bentham, James Mill and others crossed to eager disciples in Bombay and Bengal transplanting their authors' dreams of administrative and legal rationalism to a new setting, a setting, incidentally, that none of the originators of the doctrine had ever visited. For James Mill in particular, India was " a world in chaos "; despotism, both political and religious, had endowed the inhabitants with vices that surpassed " even the usual measure of uncultivated society ". All this was to be changed by the simple processes of law into a Benthamite paradise.

Now it is Dr. Stokes' central thesis that this attitude inevitably reduced the democratic element in Utilitarianism to a minimum, forcing up instead the " efficiency criterion " as the basic premise. The consequence was that, in his words, " the virtues of authoritarian government as an instrument of progress finally turned Utilitarianism into the channels of late nineteenth century Imperialism ".

James Mill defined a progressive state in terms of three main functions—administration, law, and the revenue system; and Dr. Stokes traces through the impact of Utilitarian thinking in these fields. In each case the new doctrines appeared to require the replacement of indigenous social institutions, and, at least initially, the minimum participation of Indians in the control of the country.

It is chapter two, " Political Economy and the Land Revenue ", which will be of most interest to economists; here Dr. Stokes shows us how the Ricardian doctrine of rent had a much greater impact on policy in its Indian setting than in England. The extreme view of the net return to land as a surplus suitable for absorption by the state for revenue purposes had always been hedged in with innumerable qualifications. But in its Indian application James Mill gave the doctrine an unchecked field; not merely did he advocate a tax on rent as the main source of government revenue, but he went further and demanded that the state be the sole owner of property. Under these circumstances the state could always exact the full economic rent, a situation which would prove impossible under private ownership. Practically, the policy involved the maintenance of a system of peasant cultivation and a detailed method of assessment and collection, and although it ran into insuperable practical difficulties almost immediately, the policy was not entirely abandoned until the end of the nineteenth century.

In his final chapter, Dr. Stokes deals with the " Utilitarian Legacy " and it is here that the present reviewer begins to question his analysis. He puts the case for regarding the late nineteenth century imperialism, associated especially with Fitz-james Stephens, as a direct legacy of Utilitarianism. But is it really possible to regard any policy as following inevitably from Utilitarian principles ? The term is so vague that its use tends to hinder rather than advance the understanding of British thought in the nineteenth century.

B. A. CORRY.

Nationalisation in Britain: the End of a Dogma. By R. KELF-COHEN. Macmillan & Co. Ltd. 1958. x + 310 pp. 25s.

Mr. Kelf-Cohen states that his main object in writing his book has " been to provoke some hard thinking " on a subject " befogged by doctrinaire beliefs "; he certainly attacks with vigour and success some of the more simple-minded ideas about the transformation which nationalisation was expected to bring about in industrial society, ideas which according to the Preface he himself held until experience showed that things worked out rather differently. After a brief review of socialist thought and Labour Party policy before 1945, there are separate chapters for each of five nationalised industries (including iron and steel) describing how nationalisation was brought about and the forms of organisation adopted. A further seven chapters deal with specific topics, such as the relations of Parliament and Government with the industries, the finances of the industries, the position of the workers, etc. Two concluding chapters deal with future Labour Party policy and (very briefly) possible future lines of development.

Although many specific shortcomings are pointed out, such as that of neglect of the interests of consumers in face of a monopoly of capital and labour, Mr. Kelf-Cohen stresses three basic reasons why so much has gone wrong. First, there was lack of forethought, everything being done in a hurry; secondly, the industries have been too favourably treated in the provision of capital, being allowed to have virtually whatever they wanted; thirdly, there has been confusion of control and confusion of aim. Mr. Kelf-Cohen is at his best in dealing with the problems of the relations between the nationalised industries and Parliament on the one hand and Government on the other, showing clearly both the inherent dilemmas and the danger to parliamentary government. Indeed, this latter is one compelling reason for calling a halt to further nationalisation: " The constitution and procedure of Parliament were never intended to supervise—even in a most general fashion—the complex working of enormous industries. To bring into public ownership, more and more industries, would lead to an increased stultification of Parliament, and to a greater expansion of the power of the Executive ". This matter rarely receives the attention it deserves and Mr. Kelf-Cohen's comments are timely and penetrating.

The treatment of more purely economic questions is less satisfactory. For example, price policies, which lie at the heart of so many other matters, are touched on almost incidentally, while the discussion of investment would have benefited from a little more hard thinking. It is unfortunate that a frequently polemical tone and some repetitiveness tend to obscure the value of the argument and of the considerable amount of material which is marshalled to support it.

S. R. Dennison.

The Distribution of Fruit and Vegetables. By R. L. Smyth. Gerald Duckworth & Co. Ltd. 1959. 232 pp. 30s.

The Linlithgow Committee believed in 1924 that the spread between producers' and consumers' prices for horticultural produce was " unjustifiably wide ", and the Lucas Committee of 1947 thought that " horticultural marketing illustrates in exaggerated form every weakness that can be found anywhere in agricultural marketing ". The Runciman Committee reported in 1957, however, that the existing distributive system, though not perfect, was " better than any other which could take its place at present ", Mr. Smyth, though rather sparing in his conclusions, evidently agrees much more closely with the Runciman Committee than with either of its predecessors. He favours subsidies for the improvement of grading and packing and endorses the proposal, now accepted by the Government, for the establishment of a Horticultural Marketing Council; but this seems to be the sum of his positive recommendations. He thinks that producers' marketing boards for domestic produce are " probably beneficial " in improving efficiency of distribution, but unlikely to be so active in this respect as the boards engaged in marketing produce from exporting countries in Great Britain, of whose operation he gives an interesting account. He thinks that a number of small wholesalers could with advantage be eliminated, but does not propose their elimination, and he is not sure whether there would on balance be any gain in reducing the number of retailers. He professes himself " convinced " that " ways and means must be found to maintain and improve the services rendered " by traders, but has evidently been unable to convince himself of the desirability of any positive government action to bring about this improvement.

He thinks that the forces of competition have contributed to the " existing chaos " of the primary markets, which urgently require improved road access, larger space and modern buildings, though elsewhere he attributes their condition partly to the failure to charge competitive rents. He considers that the " safest " policy for improving them would be to rebuild on their existing sites rather than move them. He does not discuss the factors determining the most advantageous location of markets, and though he seems on the whole to accept the view that the London markets attract a larger volume of produce than is consumed in their own hinterland, he gives no detailed consideration to the possible advantages of a deliberate policy to alter this state of affairs. He describes it as a " defect " in the organisation of the trade, but one that is difficult either to explain or to remedy.

The reduction of tariffs would, he says, be an " obvious way " of reducing the costs of fresh fruit and vegetable distribution, apparently because the imported produce that would then be substituted for home-grown supplies would be cheaper to distribute because better graded and packed. But he thinks growers ought always to be protected against dumping at the height of the season and appears to agree with Professor Austin Robinson that we cannot afford to permit larger imports. He would like to substitute subsidies for tariff protection, because they could be used to " promote quality rather than quantity " and limited to produce reaching approved standards; but he does not consider the resulting problems of inspection or how the subsidies should be distributed between different products. He says the usual things about the bad grading and packing of home-grown produce, briefly commends co-operative marketing and adds the customary remarks about the reluctance of growers to take advantage of it. Mr. Smyth is mainly interested in the wholesale and post-wholesale stages of the distributive process and deals rather sketchily with the problems arising at the grower's end. He does not discuss the adequacy of the protection afforded to growers by the regulations affecting commission sales or the fraudulent practices to which the commission system is sometimes alleged to give rise. He refers to the auction selling which is compulsory for the first sale of horticultural products in the Netherlands, but does not point out that these are " Dutch " auctions in which the price is not raised as the produce attracts bids but is reduced if it fails to do so.

In sum then the book, appearing as it does little more than a year after the Runciman Report, cannot be said to add very much to what was already known about the economics of fruit and vegetable distribution. The author has made a few investigations of his own, but their results are not particularly novel or striking, and his fondness for sitting on fences prevents him from drawing more than a few somewhat unoriginal conclusions. The book is, however, competently written, gives a clear description of the way in which the distributive trades function and makes their working economically intelligible. Within its limitations it is a serviceable supplement to the official reports of which most of the existing literature consists, and will be useful to introduce the subject to students making their first acquaintance with it.

E. F. Nash.

The Japanese Factory: Aspects of its Social Organization. By James C. Abegglen. The Free Press, Glencoe, Illinois, for the Center for International Studies, Massachusetts Institute of Technology. 1958. xiv + 142 pp. $4.

We are used to thinking of changes in social structure as both prerequisite for the rise of industry and consequent upon it. Here is an illuminating study of how industry can be imported into a social structure without much changing it.

In a recent year of study in Japan, Mr. Abegglen investigated 19 large factories, employing from 2,000 to 8,000 persons apiece, and spread over 10 industries. The account he gives of the organization of their work force shows how sharply it differs from the West because it retains essential principles of a rural society based on kinship, hierarchy and cult.

The selection of recruits is made according to their educational status, not their prospective proficiency in the business. When the engagement is made, it is permanent on both sides: the employee will not leave save to return to a family farm or business (or, if a woman, to marry) and the firm will hardly dismiss him save for grave misconduct. If he proves incompetent, he will be found a niche. If sales fall off, there is a margin of temporary workers who can be dismissed, but those on the establishment will be laid off only in the direst straits—in the slump of the 1930's, we are told, some of these firms had sent men home on 60 per cent. of their usual pay; in the "Dodge depression" of 1949 one firm persuaded nearly a fifth of its employees to retire by offering something more than the lump sums normally payable at the age of retirement; but generally the worker can expect to keep his job through good years and bad. In return, he owes the firm loyalty and duty. That is his incentive, not pay or promotion. Starting pay is based on age and educational status: graduates of the five crack universities get more than graduates of the newer ones. Thereafter, increments depend on age, length of service, and size of family. Some jobs with special requirements carry allowances, but these are small; we know that the girls in the early textile mills were paid piece-rates, but there is no mention of any payment by results here. Promotion, too, is closely linked with age: it is hardly possible to put a younger man over an older; nor are men moved up across boundaries set by educational status at entry. The management chart is filled with distinctively titled posts, but their functions are not delimited, and decisions are reached not by executives each within his own defined scope but through an amorphous process of consultation, which follows the pattern of the family and village conclave, and serves to avoid the unbearable painfulness of a mistake being brought home to one man. Not out of good will, but by inherent function, the firm is paternal: a substantial part of pay consists of benefits in kind, and managers bear a responsibility for the well-being of the worker's whole household that leads them to intervene in family affairs that the West would hotly reckon none of their business.

One reflects that there are counterparts to some of this in British firms before the first world war, for instance, or in British universities today, but the application to underdeveloped economies generally is the widest and most instructive. The price for the maintenance of taut collective stays and bonds is found in a productivity generally put at about a fifth of the American counterpart. At the end of his stimulating report, Mr. Abegglen considers whether and how a change may come, and he sees it as most likely to arise not from any pressure of technical requirements within the firm but through the loosening of social ties and the increased expectation of individual advancement that urbanization and education are bringing in the circumambient society.

E. H. PHELPS BROWN.

The Geography of Iron and Steel. By N. J. G. POUNDS. Hutchinson University Library. 1959. 192 pp. 10s. 6d.

The locational problems of the iron and steel industry have for long excited controversy. They have been scrutinised by writers from many disciplines and with widely varying angles of approach. The resulting literature is commonly weak either in principle or in its application to reality; and Professor Pounds' timely book deserves a wide circulation, for it combines a mastery of the history, geography and technology of the industry and a grasp of relevant economic principles.

Pounds' exceptional qualifications are evident in the regional sections where a consistently high standard is maintained. The section on Germany and France is firmly grounded on his own published work; that on the Soviet sphere has obviously benefited from his recent research while head of the Institute of East European Studies at the University of Indiana. On early American iron smelting, where no thorough locational study exists, he is able to assemble material from obscure and little known sources to produce an account of special interest. Where, as on Britain, there are good geographical studies, he modestly acknowledges and summarises them. The coverage by countries in complete, yet the account of location and known resources accurate in detail. The standard of scientific writing also is particularly fine and the opening chapter on the history of iron and steel technology probably unsurpassed as an introduction for the non-technical student. The influence of the past is shown convincingly throughout, justifying his assertion in the foreword that each of the sequent geographical patterns " has continued to exist in some shape or form, after the forces which created it had disappeared ".

Some criticism is possible on two points. One is terminology. This is impeccably correct but might on occasion be made clearer to the non-specialist. For example, the terms Thomas and Bessemer process are sometimes used in the continental sense, whereas English writers use basic and acid Bessemer process, respectively. Again, the uninitiated may not recognise Stettin in Sczeczin or Mariupol in Zhdanov. A second point of criticism is the summary of location factors in chapter III which is very compressed. If it came in a final chapter the reader would be better equipped to infer for himself the many qualifications some of the statements require. It would be an improvement, too, if there were in this chapter a closer study of measurements of locational influences and of the comparative advantages of different locations. The relevant papers are, however, quoted in the footnotes. Similarly, the inclusion of recent cost analyses would have strengthened the main conclusions on the location of the U.S. industry and, incidentally, have questioned the alleged higher costs in the West.

The book is essential to anyone who requires an accurate, up-to-date and realistic analysis of the geography of iron and steel. There is a cold appraisal of the advantages of Birmingham, Alabama, an example of that very rare and supposedly ideal type of centre adjacent to both iron ore and coking coal, but which happens to have high mining costs and to be in a region of rather low demand. The clouds of myth on the imagined value to Duluth and the Ruhr of " return cargoes " are dispersed and the troubles of the Urals-Kuznetsk combine exposed. In studies of underdeveloped countries there is a welcome balance between the weight attached to stage of advance and to physical resources. This should give comfort to the planners of India and will, it is to be hoped, cause misgivings in Portugal and Indonesia.

J. E. MARTIN.

Railroad Transportation and Public Policy. By JAMES C. NELSON. The Brooking Institution, Washington, D.C. 1959. xiii + 512 pp. $7.50.

The present troubled state of the transport industries in the U.S.A. has stimulated many studies of problems and solutions. This is the second main work to appear in 1959. (The other is Meyer *et al*, " The Economics of Competition in the Transportation Industries ", Harvard, 1959.) In part I the author sets out the " basic factors in the railroad problem ". Mainly from published data he produces many tables showing the declining share of railroads in the expansion of traffic. Airlines and buses are the winners in passenger competition and trucks are the most rapidly growing competitor for freight traffic. The familiar result is that, in spite of a high level of economic activity, most railroads have earned very low profits or even suffered losses.

Government regulation is the main reason for this state of affairs. The barriers to changes in rates and services raised by the Interstate Commerce Act and manned by the Interstate Commerce Commission have prevented the railroads from effectively competing with motor carriers for profitable traffic. Rate scales were fixed according to " value of commodity " and not according to cost of service. And it is the old story of the motor carrier's " creaming " the traffic of the railroads by taking the highly valued commodities. But government is not the only culprit. To some extent managements of railroads are also to blame for not increasing productivity as fast as might reasonably be expected.

The author must be congratulated on the collection of such a large quantity of evidence. But one hesitates to approve of his interpretations. The many indicators of cost, such as revenue ton-miles per employee and ton-miles per gallon of fuel consumed, and indicators of efficiency, such as average car-miles per train-mile, are unsophisticated and convey very little about cost or efficiency. But if his use of economic theory is sparing in dealing with the nature of the problem, it is even more lacking in part II, " Steps towards a Solution ".

Professor Nelson puts his faith in " incentive rates " or selective rate changes, that is, rate reductions where costs are low and where there is large potential traffic, and rate increases where costs exceed charges. Furthermore, there must be freedom to withdraw services—particularly passenger trains—which are unprofitable. The author does not want to abolish all economic regulation of carriers since he still fears that railroads would discriminate in an undesirable way. And he still thinks that some *minimum* rate control is necessary to prevent the destruction of competition. The author presents no theoretical or empirical evidence to support these contentions. The classical subsidy policy of welfare economics is dismissed in a sentence as possibly " wasteful ".

Within the limits which Professor Nelson has apparently set himself, he gives wise advice to managements of railroads. He points out the broad areas of traffic where they might make more money. He indicates how government regulation has been wasteful and accepts the proposals of the 1955 Cabinet Committee. To the economist this may seem to be rather timid, but to many railroadmen these are quite bold suggestions. For the reader who wants to know the facts of the decline of U.S. railroads, this book will serve as a standard source of reference for many years. It is a most useful supplement to the more analytical study of John Meyer and his co-authors.

A. A. WALTERS.

An Economic History of Transport. By CHRISTOPHER I. SAVAGE. Hutchinson & Co. (Publishers) Ltd. 1959. 216 pp. 18s.

The scope of this book is much less wide than its title might suggest. Mr. Savage limits himself to a general economic history of inland transport in Britain with a final chapter added on transport within the United States since the colonial period. The first 100 pages are devoted to transport in Britain from virtually the canal era down to the First World War. The 20 years between the First and Second World Wars are covered in a further 75 pages and the United States is disposed of in the final 25 pages. This division of space suggests that Mr. Savage was not at all clear what kind of book he intended to write and for what kind of reader. The impression is confirmed by the absence of any theme or approach.

It is not unreasonable to expect that a book with this title, by a transport economist who has already written a notable study of inland transport in the Second World War, would make some useful points on the economic importance of transport for the development of the economy as a whole. Half the book, however, virtually consists of an old-fashioned narrative of the development of the principal forms of transport down to 1914. The tremendous impact of railroad construction on the economy, for example, is not mentioned at all; while the economic consequences of railroad operations are dismissed in a few trite sentences. The blunt truth is that Mr. Savage shows an inadequate knowledge of the essential economic background to his subject. Not only does he overlook a number of very relevant specialised works, but more astonishing still, his references and bibliography do not mention any general work of British economic history written within the last 20 years. The first half of the book, in fact, bears all the signs of too much haste, too little thought and insufficient preparation.

When he writes of the inter-war years, Mr. Savage is on ground more familiar to him. The book is largely concerned in this period with the development of public control and regulation of transport in the context of increasing competition between road and rail. On this theme, the author is much more sure of his touch and gives it proportionately more space.

The final chapter on the United States is again heavily descriptive and conventional. To mention only one of its obvious weaknesses, the reader will look in vain for any discussion of the economic problems of constructing and operating canals and railroads in sparsely populated territory under nineteenth-century American conditions. Indeed, the point of including this chapter in the book is not obvious from its contents.

One is left wondering, finally, to whom this book might usefully be recommended. The charitable answer is that, for those interested in an outline of the development of transport policy between the Wars, it has, perhaps, some value, though 18 shillings is a lot to pay for so small an offering. In all other respects the book is sadly disappointing.

DAVID WIGHTMAN.

Introduction to the World Economy. By A. J. BROWN. George Allen & Unwin Ltd. 1959. 212 pp. 18s.

The purpose of this book is in the author's own words " to introduce readers to some of the salient features and problems of the world economy and to give some indication of the main ways in which economists set about the task of analysing them ". It has been written in the hope that it would encourage the readers to make a start on the subject.

The topics discussed cover a very wide field. We have first a few introductory remarks on the nature of the economic processes—production, consumption, investment and saving. This is followed by a brief analysis of the basic principles of pricing and distribution. The problem of economic growth comes next. And the book ends with an outline of the general pattern of international trade, of the factors which have brought it about and of its present organisation. The approach is entirely practical. The stress is on facts and actual problems of economic policy. All general reasoning is given in non-theoretical terms, without any graphs or formulæ, and there is only as much (or rather as little) of it as is absolutely necessary to explain the facts. Although the book contains plenty of the factual material, there are no statistical tables in it and practically no footnotes.

Thus as an introduction to economics the book follows a very unconventional line, and it may therefore have a refreshing effect on a professional reader. Whether it will encourage the beginner to make a start and pursue the subject further will depend on the beginner. If he has a practical mind and regards his books as a source of information about the world around him, he will certainly find the present one illuminating and useful. It is easy to read and gives plenty of cues for further study. If in addition to his practical interests the reader already has some knowledge of economics, the book may help him to systematise his views and to fill in the gaps. But if the beginner is of a scholarly type, more likely to be impressed by technical skill and precision of thought than by practical knowledge, he may find the book a little too much like an article in a periodical. Its broad generalisations and rough estimates may make him think that the subject is not being treated seriously. This would, of course, be a mistake. There is a great deal of very serious thinking behind even the roughest estimates, and the argument is always theoretically sound. The scholarly beginner would thus be well advised to control his first reactions and to read the book carefully from cover to cover. It will certainly do him a lot of good.

S. A. OZGA.

The Theory and Practice of Exchange Control in Germany. By FRANK C. CHILD. International Scholars' Forum. Martinus Nijhoff, The Hague. Batsford. 1958. x + 241 pp. 38s.

The theory expounded in this book is that of monopolistic price discrimination in foreign exchange markets. The practice referred to in the title is the German experience in foreign exchange control before the last war. And the theme around which most of the book has been written is the interpretation of the latter in terms of the former.

The theoretical apparatus is very crude, but it helps to bring to light the monopolistic elements in import and export restrictions. It is argued that (subject to certain familiar qualifications) the curve of demand for foreign exchange may be interpreted as the curve of its marginal valuations, and that the gains from trade are maximised at that rate of exchange at which the marginal valuation of foreign exchange is equal to the marginal costs of obtaining it. The optimum rate of exchange (from the point of view of a single country) is thus not that rate at which the curve of demand for foreign exchange intersects the curve of its supply, but that at which it intersects the *marginal* curve to the curve of supply. And as the elasticities of supply of foreign exchange from different countries need not be the same, the optimum rates of exchange in any bilateral relations with them need not be the same either. Maximisation of the gains from trade through monopolistic (or rather monopsonistic) exploitation may thus imply discrimination.

The principal thesis maintained by the author is that the German system of foreign exchange control developed gradually into a set of restrictions and regulations which enabled the government to deal with other countries bilaterally and to exploit them monopolistically in accordance with the above principles. At first, before the Nazi revolution, foreign exchange control was introduced and maintained as a defensive measure to stop the flight of capital. The general policy was deflationary and the considerations of external balance supreme. Genuine efforts were made to satisfy foreign creditors, and no discriminatory tendencies were yet visible. The situation changed completely, however, after 1933. The policy became expansionary and the control of trade and payments selective and arbitrary. In a world of low incomes and widespread unemployment, expansionary tendencies made the German market very attractive to other countries and increased Germany's bargaining power. And the selective methods of the control of trade enabled her to use this power to influence the terms of trade with individual partners and to extract from them additional credits. The degree of exploitation depended on the bargaining power of the partners. The countries of Eastern Europe and of South America were particularly keen to maintain their exports to Germany and suffered, therefore, the greatest losses as the result of adverse movements in their terms of trade. Western Europe fared much better. And the United Kingdom was even able to extricate some of her former credits.

The book makes interesting reading, and contains much useful information about various methods of foreign exchange control. A casual reader will have no difficulty in falling in with the views expressed in it. If, however, he chooses to be critical and requires to be convinced, he may be disappointed. Assertions about the effect of German policy on her terms of trade with individual countries play a very important part in the argument, but no attempt has been made to corroborate them by reference to any evidence. The reader has to accept them on the authority of the author's competence and general knowledge of the subject.

S. A. OZGA.

Problems of Economic Development. Sir William Meyer Lectures for 1955/56. By B. R. SHENOY. University of Madras. 1958. 236 pp. Rs. 10.

Mr. Shenoy in these seven lectures, revised and brought up to date after delivery, elaborates three main arguments: (i) that the Indian Second Five Year Plan is over-ambitious and should be drastically cut (or prolonged); in general he argues that planned expenditure should never exceed definitely anticipated resources; (ii) that deficit financing, beyond a rather narrowly limited point, is inflationary, has been inflationary in India, and that as such it tends to retard economic development; (iii) that the Indian rupee is overvalued, as shown by the much higher price of gold in India than elsewhere, and various other indications; a high-powered commission on the currency and exchange policy of the country is, therefore, long overdue.

Mr. Shenoy argues his cases well, both theoretically and with apposite facts and figures. On the other hand he appears to have been over-pessimistic about the prospects of securing external resources for the Second Plan, for although since he wrote there has been some reduction in the Plan's expenditure targets, this has been substantially less than he suggests as necessary, whilst external resources have been coming forward unexpectedly well. This political, rather than economic, favourable change could, however, hardly have been anticipated. What is of more importance, and what he ignores, is that there was (and still is) a strong case for optimistic gambling in drawing up the Indian plans. This is because, as he himself points out, even the Second Plan (before adjustment) merely aimed at absorbing additions to the labour force during the plan period, without contributing towards a reduction in the existing un- and under-employment. To have planned for an actual increase in un- and under-employment would have been hardly encouraging, to say the least. Such a denouement might possibly have been avoided by a readjustment of expenditure allocations, but this is a topic not discussed by Mr. Shenoy.

VERA ANSTEY.

Economic Development of Communist China. By CHOH-MING LI. University of California Press, Berkeley. Cambridge University Press, 1959. xvi + 284 pp. 56s. 6d.

This is the most thorough and authoritative economic study of Communist China that has yet appeared. The period covered is 1953–1957, that of the First Five Year Plan. Since then much has happened—the " Great Leap Forward " and the Commune Movement of 1958 and the disappointments apparent in the scaling down of the 1958 production figures in the summer of 1959. However, despite the swift changes occurring in China, it is probable that for some time to come anyone wishing to make a serious study of contemporary Chinese economic affairs will be well advised to begin with this work.

Mr. Li bases himself on official Chinese documents and reports, on the four main Peking daily newspapers and the chief economic periodicals. He examines the degree of reliability that can be accorded the Chinese statistical material published at different dates since 1949 and notes the definitions used in this material for a number of economic terms, thus bringing the interpretation of Chinese statistics one step nearer the sophisticated level of Western studies of the Soviet economy.

The author outlines the " socialist transformation " of economic life in the period of the First Five Year Plan, the behaviour of prices and the value (which he holds to be scanty) of the official price indices. This is followed by a discussion of the course of industrial and agricultural development in these years. Leading on from this he examines the structure and growth of the national product and concludes that the official estimates of the average rate of increase of net national product during the First Five Year Plan were inflated. The chapter on net capital formation includes an interesting comparison between Chinese and Indian capital coefficients which, it is maintained, " with proper allowance for the difference in definition . . .

would not be so different as they appear " (p. 137). The final chapters are devoted to a consideration of China's internal and external financing and to a discussion of future prospects.

Throughout, the author concentrates on what has happened rather than on descriptions of how it has happened. This, however, is not a criticism, for the topics he has covered are very substantial in themselves, and he is to be congratulated on the thoroughness and solidity of his work.

AUDREY G. DONNITHORNE.

Queues, Inventories and Maintenance. By P. M. MORSE. John Wiley & Sons, Inc., New York. Chapman & Hall Ltd. 1958. ix + 202 pp. 52s.

This is the first of the Wiley series, " Publications in Operations Research ", sponsored by the Operations Research Society of America. It also enjoys the distinction of being the first book to appear in English on its subject: the application of probability theory to queueing and related situations involving flow and congestion. These situations are remarkable in extent and variety, and in recent years many problems have been investigated: the flow of machines through a repair shop, ships berthing, lorries unloading, aircraft at landing fields, units on an assembly line, traffic at crossroads, and so on. One of the first important studies dates back to 1908 in connection with problems of telephone operation.

The book does not pretend to be an exhaustive treatise on queueing theory. It fulfills its purpose well of giving a general description of the subject, with definitions of terms and an outline of the various fields of application of the theory. The mathematics is at the elementary calculus level and only the simplest analytic techniques are used. To a mathematician the result is a development which will occasionally seem clumsy. But to the operations research practitioner with slight mathematics but plenty of determination (to whom the book seems to be addressed) the approach will be welcome. New to this field, he will soon begin to find his way around, and can start applying the techniques to his own problems.

Probability concepts are introduced in a short chapter: this is the least successful part of the book, and the reader would be well advised to have acquired his elementary statistical theory elsewhere. The topics then discussed include: single and multiple channels, finite and infinite queues, exponential and non-exponential distributions of service and arrival-times, transient behaviour, priorities, inventories, machine maintenance. Optimization problems involving cost considerations are treated.

F. G. FOSTER.

SHORTER NOTICES

Histoire des théories économiques spatiales. By CLAUDE PONSARD. Centre d'Etudes Economiques. Librairie Armand Colin, Paris. 1958. 202 pp. Fr.1200.

The book provides a systematic exposition of the development of the theory of location during the last 150 years. It deserves the attention of every student interested in the subject. After an introduction in which the origins of the theory are explained, the author examines critically the contributions of Von Thünen, Weber, Predöhl, Palender and Lösch. More recent writers are treated more summarily. A rigorous exposition of the models discussed in the text is given in a mathematical appendix. The book has an extensive bibliography.

Economics of the New Zealand Maori. By RAYMOND FIRTH. Government Printer, Wellington. 1959. 519 pp. 50s.

Professor Firth's pioneering contribution to the study of economic anthropology, which was first published in 1929, now makes its welcome reappearance in a revised edition. The principal revisions are in the opening chapter on " The Study of Economic Anthropology " and in the penultimate chapter on " The Economic Aspect of Cultural Change ".

Essays on Japanese Economy. By SHIGETO TSURU. Economic Research Series, Vol. II. Hitotsubashi University. Kinokuniya Bookstore Co. Ltd., Tokyo. 1958. 241 pp.

Professor Tsuru is an outstanding Japanese economist, and the best known abroad, because of his residence in the West and ability (sadly rare among Japanese scholars) to use English with real facility.

This is a collection of his writings, six on Japan's current problems in 1949-57, two on earlier "historical" themes. They are straight reprints; unfortunately, perhaps, Professor Tsuru elects not to add postscripts or corrections in the light of hindsight or subsequently available information. But so little comes out directly into English from Japanese scholars themselves that we may warmly welcome this solid material, and hope for more.

China's Gross National Product and Social Accounts, 1950-1957. BY W. W. HOLLISTER. The Free Press, Glencoe, Illinois. 1959. xxv, 161 pp. $4.

This work consists of some interesting calculations of the size and composition of China's gross national product. It is based on official Chinese statistics with certain adjustments, notably in the agricultural sector. The author, while realising the weaknesses inherent in such an approach, gives perhaps too little weight to them and may well be over-optimistic about the reliability of his estimates. The exposition is somewhat confused and makes unnecessarily difficult reading.

Resale Price Maintenance. By SØREN GAMMELGAARD. The European Productivity Agency of the Organisation for European Economic Co-operation. 1958. 114 pp. 8s.

This report provides a concise discussion of the most important aspects of resale price maintenance. A descriptive section deals with the different types, methods of introduction and enforcement, and the age and extent of the practice; this is supplemented by a survey of the relevant legislation in the O.E.E.C. countries, Canada and the United States. There follows a survey of the motives which are attributed to businessmen who practise resale price maintenance. Theoretical analysis is used to examine the effects on industry and trade and the conclusions are supported from the available --though admittedly scanty—empirical evidence.

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